Debating Roman Demography

Edited by

Walter Scheidel

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INTRODUCTION

Walter Scheidel

Ancient demography has finally arrived. A mere decade ago, Werner Dahlheim concluded a survey of scholarship on the subject with the sombre prediction that ancient history 'was probably a long way from a renaissance of population studies' (Dahlheim (1989) 317). He could hardly have been more mistaken. As Glen Bowersock put it in a paper originating as a contribution to the Presidential Forum of the Annual Meeting of the American Philological Association of 1996, recent work on ancient demography 'can leave us in no doubt that this ancillary discipline has finally taken a respected place in classical scholarship . . . demography is there to stay' (Bowersock (1997) 373).

And indeed, ancient demography has been quick to acquire the traditional trappings of academic respectability: a textbook (Parkin (1992); its own chapter in the second edition of the venerable *Cambridge Ancient History* (Frier (2000)); book-length bibliographies of earlier scholarship (Suder (1988, 1991), Corvisier and Suder (1996)); surveys of recent and ongoing research (Golden (2000) and below, Chapter 1); and, last but not least, specialised conferences and their proceedings. The 'Premier colloque international de démographie historique antique' at the Université d'Artois in Arras, France, in November 1996 (Bellancourt-Valdher and Corvisier, eds. (1999)), and the 'Incontro internazionale di studio: demografia, sistemi agrari, regimi alimentari nel mondo antico' at the Università degli Studi in Parma, Italy, in October 1997 (Vera, ed. (1999)), were among the first gatherings specifically devoted to this area. The present volume owes its existence to a similar event, the 'First Finley colloquium on ancient social and economic history', entitled 'Population size and demographic structure in the ancient world', which I organized at Darwin College, Cambridge, in May 1997.

The following chapters, four of which are based on contributions to the Cambridge meeting, are meant to illustrate recent progress and abiding problems in the exploration of ancient populations. They cover a wide range of different methodological preferences from the

scrutiny of ancient texts and archaeological remains to expressly theoretical, comparative and interdisciplinary approaches, as well as some of the most salient issues and controversies in the field.

My opening chapter sets the scene with a critical survey of modern attempts to shed light on key features of the formal demography of the Roman world. After opening my discussion with a few historiographical observations on the academic development of the field, I focus on the central issues of mortality, fertility, migration and population size, followed by some preliminary thoughts on the economic and ecological dimension of demographic research.

The second chapter concludes Brent Shaw's series of pioneering studies of seasonal features in Roman demography (Shaw (1996, 1997)). The seasonal birthing cycle of Roman women, never before explored in any detail, is of interest beyond the confines of ancient history: a subject that has long engaged experts in more recent demography is now enriched by a discussion of what appears to be the oldest pertinent quantifiable evidence in the world.

The remaining papers focus on questions of population size and development. Elio Lo Cascio adds another brick to the edifice of his controversial revisionist model of Roman manpower. Elaborating on previous studies, he seeks to strengthen his central thesis that the number of Roman citizens was much larger than conventionally assumed with reference to the recruitment of soldiers in the last three centuries BCE: in his view, the current 'low' estimate of the size of the Roman population implies improbably high levels of military mobilization. While Lo Cascio's alternative reconstruction of Roman history has yet to find acceptance, and has indeed already been criticized by some (including myself), this particular point highlights a weakness of standard accounts that cannot fail to attract attention. It is challenges to received wisdom such as this that fuel scholarly controversy, refining our understanding of ancient population history.

In the next contribution, Bruce Frier approaches the question of population size from a new angle. Uniquely among scholars who have traditionally insisted on pondering supposed demographic decline, he considers the possibility of overpopulation in the early Roman empire and reminds us of the link, ignored in existing work on 'the ancient economy', between demographic and economic development. Novel in its argument as well as in its sophisticated comparative and theoretical perspective, Frier's study provides ample food for thought and forces us to rethink time-honoured views.

The final chapter returns to that recent favourite of ancient historians with an interest in population: in much the same way as Egypt is the gift of the Nile, ancient demography is the gift of Graeco-Roman Egypt. Richard Alston reconsiders the demise of the ancient city in late antique Egypt. This discussion, in turn, has a bearing on other important problems, from the causes of urbanisation in early Roman Egypt to the definition of the 'end' of classical antiquity.

The four thematic chapters share a strong comparative and interdisciplinary emphasis. Shaw interprets ancient seasonality against the background of comparative evidence, and considers scientific research into its causes; Lo Cascio supports his case with evidence of militarisation in other pre-industrial societies; Frier derives both the questions he asks and the concepts he applies from historical scholarship on the more recent past, especially on early modern France; Alston's discussion is informed by comparative evidence of the implications of phenomena as diverse as epidemics and monasticism.

This general tendency is by no means accidental. The lack of ancient statistics is not merely a cliché but a very serious obstacle to our understanding of Greek and Roman demography. And we would be deluding ourselves if we believed that demography is a marginal and thus inessential area of inquiry. To give an extreme example, if Lo Cascio's alternative model were correct, Roman history as a whole would have to be rewritten. It boggles the mind that at the brink of the twenty-first century, after generations of critical scholarship, a debate over fundamentals of this kind is still possible. More than anything else, this highlights the extent to which our enquiries into ancient populations are frustrated by the nature of our evidence. As a consequence, research in Greek and Roman population history must move beyond the narrow confines of the ancient sources. For all its methodological pitfalls and practical shortcomings, a comparative approach offers a less defective alternative to even less palatable options. Because of the constants in human reproductive biology and, to some extent, in human mating behaviour and the interaction between humans and disease agents, demography is better suited to cross-cultural evaluations than many other branches of historical analysis. Furthermore, it should not be forgotten that ancient historians are not necessarily reduced to borrowing from their rich relations: in those cases where quantifiable ancient evidence does exists, it helps extend the scope of historical enquiry very considerably, from the end of the Middle Ages all the way back

to the beginning of the Christian era. Shaw's and my own work on seasonal aspects of ancient demography are only one example.

The chapters by Shaw, Lo Cascio, Frier and Alston grew out of their papers for the Cambridge meeting. Some other pieces could not be included here because they formed part of ongoing research that will be fully developed elsewhere: in particular, this holds for Dorothy Thompson's preview of exciting new papyrological material from Ptolemaic Egypt that she will discuss in a forthcoming book co-authored with Willy Clarysse, and for John Patterson's paper on the contribution of archaeology to the population history of Roman Italy, which will be part of his forthcoming book on Roman Italy. In characteristic fashion, Robert Sallares confronted the audience with 'A Greek historian's view of Roman population studies', elements of which have found their way into his forthcoming study of disease and demography in Roman Italy (Sallares (forthcoming)). My own conference paper summarised the gist of my recent investigations of Roman slave demography (Scheidel (1997c, 1999c, d, forthcoming e)).

The colloquium greatly benefited from all those who delivered papers or contributed to the discussion and informal exchange of ideas outside the sessions: as speakers and chairmen, Richard Duncan-Jones, Keith Hopkins, Wim Jongman, Dominic Rathbone, Richard Saller, and Peter Garnsey (the last *in absentia*); among the other participants, Riet van Bremen, Graham Burton, Jean-Nicolas Corvisier, Ben Isaac, Sir Geoffrey Lloyd, Arnaldo Marcone, Neville Morley, Henrik Mouritsen, John North, Graham Oliver, Helen Parkins and Rens Tacoma. Peter Laslett and Richard Smith, of the Cambridge Group for the History of Population and Social Structure, provided invaluable comparative input. I would also like to take this opportunity to thank Darwin College and the Faculty of Classics in Cambridge for funding, all conference participants for their good cheer in the face of financial austerity, and the contributors to this volume for patiently tolerating delays and editorial prodding. I am indebted to Wim Jongman for his invitation to publish the present volume in this series, and grateful for his and Ian Morris's comments on several of our chapters.

CHAPTER ONE

PROGRESS AND PROBLEMS IN ROMAN DEMOGRAPHY

Walter Scheidel

Demography and Roman history: a work in progress

A general survey of the principal perspectives, questions, methods, findings and controversies in the study of Roman population history has long been missing.[1] On this occasion, a truly comprehensive discussion remains well beyond our reach: situating the demographic features of Roman history in their social, economic, cultural, biological and modern academic context, a full overview would require a substantial monograph of its own.[2] In this chapter, I will focus more narrowly on the 'hard core' of demography, of a field that has been defined as 'the scientific study of human populations, primarily with respect to their size, their structure and their development'.[3] This 'hard core' is the subject of what is conventionally termed 'formal demography', defined as 'the treatment of quantitative relations among demographic phenomena in abstraction from their association with other phenomena', as opposed to the more broadly conceived area of 'population studies' which is devoted to the relations between demographic events and social, economic or cultural phenomena.[4] In keeping with the methodological preferences of the other

[1] Corvisier (1996) provides a bibliographical discussion of recent work on ancient Greek demography; see also Brun (1999). For an outline of the current state of historical demography in general, see Saito (1996).

[2] Nor is this the place for an assessment of the nature of the different categories of pertinent evidence from the Roman period or an introduction to the methods of professional demography. Parkin (1992) addresses both issues in an accessible critical survey directed at an audience of Roman historians and classicists. I will give a more wide-ranging general overview in a volume currently in preparation for the 'Key Themes in Ancient history' series of Cambridge University Press (Scheidel (forthcoming b)).

[3] van de Walle (1982) 101.

[4] Newell (1988) 3–4. For a similar differentiation, e.g., Schofield and Coleman

papers in this volume, I shall place particular emphasis on quantitative and comparative approaches. Constraints of space forestall consideration of the burgeoning field of Roman family history.[5] While studies of the Roman household, marriage, gender and childhood largely rely on non-statistical textual sources, they are increasingly informed by an appreciation of the underlying regime of mortality and fertility and its constraints on and implications for social, legal and cultural practice. This process has not been confined to the qualititative branches of Roman population studies: recent work on formal demography is beginning to have a significant impact on the way ancient history is done.[6] Future research on a variety of issues from economic development to the evolution of legal institutions will increasingly be shaped by a demographic perspective.[7] A demographic framework is likewise of great significance for the interpretation of the material record, and helps put the study of ancient medicine in a broader context. But it is true that much of this lies still ahead: for this reason, as well as in view of recent advances, it is not merely a cliché to consider the turn of the millennium the right moment to take stock of the field of Roman demography, to identify real progress and abiding problems, and to suggest pathways for further research. In this, I take my lead from the choice of topics covered in what is arguably the single most important and influential contribution to historical demography, Wrigley and Schofield's population history of England, ranging from population size, age structure,

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(1986) 5–6. See also Caldwell (1996) 305–8 for various ways of defining demography, and cf. Greenhalgh (1996) for a historical perspective. The contributions to the anniversary issue of the journal *Population Studies* (vol. 50, no. 3, 1996) provide an excellent introduction to the main concerns of the discipline.

5 The main book-length studies of the past fifteen years include Rawson, ed. (1986); Dixon (1988); Wiedemann (1989); Andreau and Bruhns, eds. (1990); Rawson, ed. (1991); Bradley (1991); Evans (1991); Gardner and Wiedemann (1991); Kertzer and Saller, eds. (1991) chs. 2–7; Treggiari (1991); Dixon (1992); Saller (1994); Krause (1994–95); Evans-Grubbs (1995); Friedl (1996); Rawson and Weaver, eds. (1997); Gardner (1998). Cf. Bannon (1997) for a different perspective. For late antiquity, see also the substantial articles by Shaw (1984, 1987b). Bradley (1993, 1998) offers critical overviews of recent work. For older bibliography, see Krause (1992) (already outdated by the concurrent surge in major publications). More recently, historians of ancient Greece have begun to show similar interests: e.g., Ogden (1996); Pomeroy (1997); Cox (1998); Patterson (1998). Needless to say, any appraisal of gender studies would require even more extensive references.

6 What may be the best known example concerns classical Greek, namely Hansen (1985), on the interplay between Athenian demography and political participation.

7 See below, 'Demography and economy'.

migration, fertility, mortality, nuptiality, seasonality and mortality crises to the economic setting of long-term trends on fertility and mortality and an attempt at 'a dynamic model of the relationship between population and environment'.[8]

Roman demography past and present

Before we turn to these issues, a few words on the historiographical background are in order. The conceptualisation and ideological evaluation of demographic conditions in classical literature still awaits serious treatment. So far, scholarly attention has centred on attitudes to culturally determined demographic events, especially various aspects of fertility and fertility control.[9] By contrast, Greek and Roman views on demographic change or the correlation between population and social, political and economic development in general have rarely been examined in detail.[10] This neglect not only narrows our vision of the intellectual tradition of the classical world. More importantly, a proper appreciation of literary conventions and the rhetorical-topical context is essential for the evaluation of individual claims encountered in our sources. At the very least, it would help dissuade modern observers from literal readings of decontextualised references.

In a brief new overview, Ulf can do no more than touch on a number of highly pertinent features from the ancient belief in the desirability of large population numbers, the resultant pro-natalist ideology, and the common association of moral decline with demographic contraction—motifs which still exert undue influence on the imagination of modern students of ancient population history.[11] In the Middle Ages and the early modern period, the ancient tradition was primarily instrumentalised in underpinning pro-natalist and

[8] Wrigley and Schofield (1989).

[9] See Eyben (1980/81) 19–74 on Greek and Roman attitudes to fertility control and family limitation, and Nardi (1971) for an exhaustive survey of abortion. On ethical aspects, see, e.g., Cameron (1932); Dölger (1934); Grahay (1941); Gorman (1982); Grassl (1982) 58–64; Feen (1983); Carrick (1985). Childlessness: Lambert (1982); Grassl (1982) 56–8. Christian celibacy: Brown (1988); Forlin Patrucco (1999). On widows, see Krause (1994–95), esp. vol. 1, 58–66 on remarriage.

[10] A series of papers on 'population policy in Plato and Aristotle' in *Arethusa* 8 (1975) is the main exception. See furthermore Moreau (1949) 603–9; Gallo (1980, 1991); Ampolo (1985); and very briefly Vidal (1994) 9–21.

[11] Ulf (1999) 481–4. See below, n. 163–7.

growth-oriented exhortations while utopian models could be evoked in support of the alternative ideal of tightly controlled stationary populations.[12] In this connection, the Augustan marriage legislation attracted a disproportionate share of attention, prompting calls for the enactment of similar measures in the present: Colbert's adaptation of the *lex Papia Poppea* in the second half of the seventeenth century is the best-known example.[13] A century earlier, Bodin had warned of the dangers of the 'dénombrement' of the population and advocated the restoration of the Roman office of censor.[14]

The gradual emergence of a more critical attitude is usually traced back to the eighteenth century. However, given his reluctance to disbelieve ancient sources except in the most flagrant instances of improbability, David Hume's famous *Discourse concerning the populousness of the ancient nations* of 1752 represents rather limited progress. Even so, it drew fire from Robert Wallace in his methodologically reactionary 'Dissertation on the number of mankind in ancient and modern times' of the following year, in which his defence of recorded population figures went hand in hand with the traditional linkage of demographic developments to moral decline and luxury.[15] It is tempting to conclude that in the long run, and in a way up to the present, Hume's position of hesitant scepticism prevailed.[16] Even though in academic circles, Roman population history has long since come to be regarded as an object of critical study rather than an authoritative reference point for contemporary concerns, in some respects ancient preoccupations and prejudices are still very much alive. More often than not, modern discussions of Roman population size and

[12] Ulf (1999) 484–5. In general, see Stangeland (1904).

[13] E.g., Mombert (1931) 483–4. See also Frier in this volume, on French worries about depopulation.

[14] Demandt (1984) 115. Montesquieu traced the supposedly dramatic depopulation of the ancient world to Christian ascetism *and* luxurious living (Demandt (1984) 140). The same paradoxical combination was favoured by J. C. Krause in 1789, namely 'Ehelosigkeit, die Möncherey, die Üppigkeit, die Ausgelassenheit der Sitten' (ibid. 157).

[15] On this debate, see Cambiano (1984). It deserves notice that despite his deflation of reported numbers, Hume was sceptical of the alleged depopulation of the Roman provinces (Demandt (1984) 128). The pre-Humean belief that the ancient world was more densely populated than in later periods has a parallel in Hellenistic and Roman assumptions about the depopulation of post-classical Greece: see Alcock (1993) 25–32 for references. In both traditions, depopulation is correlated with a decline in military power which in turn is associated with a decline in moral standards.

[16] Notwithstanding subsequent attempts to defend every recorded figure on general principle: Ulf (1999) 488.

fertility deliberately or subconsciously still mirror the value judgements of ancient commentators.[17]

In the nineteenth century, a few scholars went a step further in their attempts to reconstruct, or rather calculate, Roman demographic conditions by means of quasi-'scientific' extrapolation. Consideration of the relationship between population size and likely carrying capacity made it easier to assess the plausibility of recorded figures. In a similar way to Hume's critique, this approach not only exposed untenable statements in ancient sources but also served to check the arbitrariness of modern interpretations. A comparison between Dureau de la Malle's mathematical evaluation of Roman slave numbers and Blair's more text-bound study of the same subject that had appeared only a few years earlier illustrates both of these points.[18] Despite their obvious shortcomings, such as excessive reliance on schematic schedules of agricultural productivity, extrapolations from carrying capacity constituted a major advance from reverent philology to parametric modelling. In the late nineteenth century, this approach was brought to full fruition by Karl Julius Beloch.[19] In addition, a few pages of his pioneering *Bevölkerung* of 1886 already foreshadowed some of the central issues of the study of Roman demography in the late twentieth century, from the use of comparative evidence of age structure to the value of the mortality schedule suggested by Ulpian and the distortion of epigraphic age records through age-rounding and selective commemoration.[20] While lacking the requisite statistical techniques

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[17] See below, n. 163–7.

[18] Blair (1833) 15–16 reckons with a proportion of 'three slaves to one freeman' in late Republican and early imperial Italy, an idea derived from a few huge household totals reported in ancient sources. Dureau de la Malle (1840) 252 dismisses the resultant population total for Roman Italy of close to 28 million for Blair's failure to account for carrying capacity, which renders this figure impossible. His own calculation, a mixture of reported population figures for the free Italian population and assumptions about maximum carrying capacity, yields a much more plausible total of 5 million in 225 BCE, not far from Brunt's total of 4 million at that time (Brunt (1987) 60). This highlights not only the conceptual leap between two works published in 1833 and 1840 but also the lack of significant further progress ever since.

[19] Beloch (1886). Among subsequent contributions, one might single out Beloch (1897, 1899a, 1903, 1913). Gallo (1990) provides a detailed discussion of his contribution to ancient population history and the reception of his work in the academic community. See also briefly Bowersock (1997).

[20] Beloch (1886) 41–54. The importance of this section is rightly stressed by Gallo (1990) 139–40, who also notes Beloch's abiding interest in modern demography, apparent in the marginal notes in his 'Handexemplar' of the *Bevölkerungsgeschichte*.

to tackle most of these problems, Beloch deserves credit for raising questions that would not receive proper treatment until the 1960s. His interest in comparative data and new statistical methods never faded in his later years.[21]

Perhaps unsurprisingly, peer resistance to his unconventional approach—already anticipated in the preface of the *Bevölkerungsgeschichte*—proved sufficiently determined to ensure successful containment. The positivistic Mommsen school, dominant in Beloch's day, remained hostile to his modernising inclinations and recourse to the repertoire of the nascent social sciences.[22] In terms of its methodological outlook, Seeck's critique is a classic statement, attacking Beloch's 'numbers games' based on 'conjectural statistics'.[23] Long excluded from the German market, Beloch failed to establish his own tradition of ancient historical demography: his Italian disciples, ready to emulate him in other areas, showed little interest in their maestro's distinctive brand of population studies.[24] Ettore Ciccotti, the editor of a volume including an Italian translation of Beloch's main demographic work and some of the responses it had provoked, chose to preface the text with a one-hundred page indictment of his methods and results.[25] On this occasion, Beloch appeared as the prototype of a historian who risked to abandon proper philological foundations in favour of a more general vision of historical phenomena. Despite his own Marxist leanings, Ciccotti's criticism betrays core beliefs of philological history: instead of embarking on conjectural numbers games (another term for parametric modelling), the histo-

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[21] E.g., Beloch (1897, 1899b); and see below on his Italian population history.

[22] See Beloch (1886) V–VIII, a mixture of *captatio benevolentiae* and prophetic abstract of subsequent reactions. For his position vis-à-vis German academe, see Christ (1989) 248–85 and (1990); Momigliano (1994) 97–120. Mommsen's damning verdict on his earlier work was of lasting impact (Christ (1990) 181 n. 6), despite Eduard Meyer's sympathy—Gallo (1990) 119–121; Christ (1990) 182—, which proved of little import in the face of the power of Mommsen and his disciples (cf. also Bowersock (1997) 374 n. 6).

[23] Seeck (1897), esp. his methodological credo 175–6, and in a similar vein Kornemann (1897), with the reply by Beloch (1897), in which he demolishes Seeck's polemic, offers the incontrovertible riposte that even if we avoid stating numbers, we always have them in our minds, and emphasizes the necessity to know about medieval and modern population history and to be competent in political economy and statistics. See Gallo (1990) 148–51 on this exchange.

[24] Momigliano (1994) 112–3.

[25] Ciccotti (1909), analysed by Gallo (1990) 124–8, esp. 125 n. 29: 'un vero e proprio tentativo di stroncatura, una requisitoria totale contro il metodo e i risultati dell'autore della *Bevölkerung*'.

rian of classical antiquity ought to focus on the study of *qualitative* aspects of population issues, of supposedly well-documented social practices that account for demographic developments. However, this approach could only be a dead end—for 'whatever their form and quality, statistics are the lifeblood of demography, and nothing can substitute'.[26] Thus, 'in the end, demography without numbers is waffle'.[27]

Beloch's lack of influence is rendered all the more remarkable (and embarrassing for his own profession) by the posthumous publication of his three-volume study of the population history of Italy which even today still counts as a standard work of reference.[28] One would be hard pressed to name another ancient historian of whose contribution to another field the same was true. In any event, resistance to a social scientific approach to ancient population history went back a long way. Boeckh's defence in the 1810s of fantastic slave numbers (such as the total of 400,000 reported for Athens) is only one example.[29] Aside from ideological preconceptions that modern interpreters can often be shown to share with ancient authors, including an instinctive preference for large population numbers and high fertility, a superficially more innocuous but no less pernicious bias also seems to have been at work, namely the desire to revere and trust the classical tradition.[30] Arguments from probability were consequently viewed as suspicious, as in Seeck's dismissal of Beloch's work, urging 'respect for the evidence', extolling 'the firm ground of the source tradition' and claiming that in different periods, very different things

[26] Frier (1992) 383.

[27] Schofield and Coleman (1986) 4.

[28] Beloch (1937–61); cf. Bellettini (1987) 12, who stresses that every study of Italian demography is indebted to Beloch's work and that it has until recently provided an obvious starting-point. Another work on the population history of Europe in general remains unfinished: Gallo (1990) 146 n. 77. Beloch's article of 1888 on the Italian population in the sixteenth to eighteenth centuries was still not superseded when reprinted in Cipolla, ed. (1959). Compare the praise of Braudel (1995) 395 n. 194. For further references, see Momigliano (1994) 109. Beloch embarked on this ambitious project immediately after completing the *Bevölkerungsgeschichte*; see Momigliano (1994) 100 for his first programmatic article in 1887.

[29] See the scathing comments by Finley (1998) 93, 99. It deserves notice, however, that in his own day Böckh could actually be regarded as a sceptic and attacked for his critical approach to ancient evidence: see Ungefehr-Kortus (1999).

[30] For a simultaneously amusing and depressing overview of modern attempts to believe and extrapolate from Caesar's population figures for Gaul, see Henige (1998b). Recent defences of annalistic census figures for the first century of the Roman Republic belong in the same category: Ward (1990); Hinard (1997a) 109–10.

are possible.[31] A not altogether dissimilar mindset is reflected in the programmatic inaugural lecture of A. H. M. Jones in 1948, in which he disposes of Beloch's and his followers' re-interpretation of the meaning of the Augustan census figures (as referring to all Romans instead of adult men only) with the remark that 'as, however, their main argument is that the leap from 910,000 in 70 BC to 4,063,000 in 28 BC is otherwise unaccountable, they may be safely ignored'.[32] One wonders why the logical implications of either position were not supposed to matter.

For a long time, philology carried the day. In its early turn away from social science approaches, the study of ancient population history shared the fate of other strands of Greco-Roman history, such as ancient economic history. Ian Morris, in his new introduction to Finley's seminal book on the ancient economy, reminds us of this missed opportunity.[33] The vivid interest in antiquity displayed by the founding fathers of sociology from Karl Marx to Max Weber was rarely reciprocated by professional ancient historians, their work often ignored. Those willing to take Weber seriously, like Hasebroek, found themselves sidelined. When Moses Finley, reared outside the classics community, finally succeeded in putting sociological concepts on the map, all he was doing was to help ancient history catch up with mainstream history, with its interest in the analysis of social structures and economic forces and its willingness to borrow methods from the social sciences. Even so, for all his efforts his impact on the discipline has remained very limited. In this regard, Morris's assessment of the current situation is equally applicable to the study of ancient demography: 'Ancient historians are still chiefly found outside university history departments, in classics departments in north America, or in ancient history departments in Europe. They tend to go to different conferences than the ones attended by modern historians, to publish in different journals, and almost to speak a different lan-

[31] Seeck (1897) 163–4: 'Aber leider fehlen ihm der sichere Takt in der Schätzung der Quellen und namentlich der Respekt vor dem Ueberlieferten. Immer ist er bereit, das Bezeugte für "unmöglich" zu halten, und übersieht dabei ganz, daß in verschiedenen Epochen auch das Mögliche sehr verschieden sein kann. So schwebt er im freien Aether über dem festen Boden der Ueberlieferung "und mit ihm spielen Wolken und Winde".'

[32] Jones (1948) 7. On this issue, see below, n. 209–11. Even the most ardent opponents of Beloch's view now seem compelled to argue from plausibility and not just from sources alone: see Lo Cascio in this volume. This is an area where it is not difficult to define and measure 'progress'.

[33] See Morris (1999) XXIII–XXV.

guage. As late as in the 1970s, the vision of historiography as the handmaiden of philology still dominated the field. (. . .) Ancient historians effectively skipped the stage in the development of modern historiography when social and economic questions tended to dominate the agenda and moved straight from philology and politics to cultural poetics.'[34]

In this environment, demography has faced an uphill struggle. After Beloch, ancient population studies entered a phase of regression. Such research as was undertaken largely returned to the positivistic interpretation of literary and epigraphic sources.[35] Up to the 1960s, this line of research remained virtually untouched by exciting new developments in the historical demography of the more recent past. It was only with what Shaw has rightly called 'a watershed in modern approaches to ancient history', the demographic studies of Keith Hopkins in the 1960s, that the models and findings of mainstream population history were finally introduced to the discipline.[36] Since then, demography has contrived to influence our vision of the ancient

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[34] Ibid. XXIV–XXV. In Sallares's harsh words, 'the two prime reasons for the backwardness of ancient history as a subject' are the fact that most practitioners in the field 'are trained in university faculties whose main raison d'être is classical literary criticism', and its 'cult of positivism and inductivism': Sallares (1991) 37; cf. also 5–6, 417. One might add that in continental Europe, owing to the insulation of ancient history from the literary criticism that dominates Anglo-Saxon 'Classics', Morris's last shift has barely begun. In Germany, closer contacts between ancient and modern history were facilitated by an institutional framework separating classical philology from history but obstructed by personal preferences; in fact, specialised ancient history departments tend to separate ancient historians from other historians in much the same way as much as classics departments do in the Anglo-Saxon world. In this environment, the nineteenth-century cult of non-literary evidence replaced the literature-centred approach of classics, with even less palatable consequences: whereas the literature-based field of classics is at least open to new developments in cultural studies and literary criticism, the German brand of document-centred ancient history dooms its practitioners to sterile isolation. Härke (1995), a trenchant account of the German tradition of archaeology, is fully applicable to ancient history.

[35] Suder (1988) gives an exhaustive bibliography of these publications. Well-known studies include Burn (1953); Hooper (1956) and Moretti (1959), exploiting Latin tombstone inscriptions, and the mind-numbing compilation of most ages recorded in Latin epitaphs by Szilagyi in *Acta Archaeologica Academiae Scientiarum Hungaricae* 13–19 (1961–67). The work of R. Etienne and the frequently cited piece by Alföldy (1972) also belong to this category of methodologically unsophisticated work. Thanks to the superior quality of their sources, Hombert and Préaux (1952), analysing the census returns of Roman Egypt, is a notch above the rest. Attempts at synthesis were rare: see Boak (1955a), reviving the thesis of late Roman depopulation, and Russell (1958), a more wide-ranging work with a similar focus.

[36] Hopkins (1966) (drawing on (1963) 14–26) marks this watershed. Hopkins's approach is discussed by Shaw (1982), esp. 24–31 on demography (quote from 24).

world without ever outgrowing its liminal status. What progress has been made over the past thirty-five years must be credited to a mere handful of self-taught amateurs of historical demography. The old hostility may be gone: Bowersock's verdict, quoted at the beginning of the introduction, seems fairly representative of the profession.[37] However, acceptance of results does not necessarily translate to interest in the process of discovery: it catches the eye that even crucial findings have rarely if ever been criticised or challenged. Comparable indifference would be unthinkable in almost any other branch of ancient history.[38] This particular kind of passive acceptance is therefore at best a mixed blessing: while ancient demography, in Bowersock's words, may well be 'there to stay', persistent reluctance to engage with its methods and concepts will condemn it to a marginal existence.[39] Current shifts into qualitative, cultural history make this the most likely outcome. But all is not lost. The study of ancient demography has come a long way since Beloch's abortive experiment. Regardless of further progress or retrenchment, the advances of the past few decades will indeed be there to stay.

Evidence and models

The first systematic surveys of ancient source material that can be used for demographic purposes did not appear until the 1990s.[40] By now it has become possible to identify three major strands in academic approaches to this kind of evidence. Each of the positions set out below is cast as an ideal type in order to highlight its differences from competing perspectives. In practice, of course, they rarely occur in pure form.

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[37] Bowersock (1997) 373. Even so, it is worth noting the relative neglect of the subject in the essays in Thomas, ed. (1997), a collection explicity devoted to 'recent work and new directions' in ancient history.

[38] For the few exceptions, see Parkin (1992) 27–41, 75–84 on Frier (1982) (a ten-year gap); Martin (1996), on Saller and Shaw (1984) (a twelve-year gap), with Rawson (1997); Aubin (forthcoming), on Shaw (1987a) (a thirteen-year gap); Scheidel (1996e) and Scheidel (forthcoming a) ch. 2, on Bagnall and Frier (1994).

[39] For a statistical assessment of the structural inertia of classics and ancient history in general, see Scheidel (1997b).

[40] The most detailed account to date is that of Parkin (1992) 4–66. See also Saller (1994) 13–22; Parkin (2000); Frier (2000).

First, numbers reported in ancient texts or derived from samples of documentary and osteological evidence may be interpreted in a straightforwardly literal, positivistic fashion and without reference to extraneous models or comparative evidence. This approach, though common in the past, is invalidated by the specific biases inherent in much of the available data, such as the ignorance, indifference or hidden agenda of ancient authorities, preference for symbolic numbers in literary texts and for rounded numbers in epitaphs and other documents, cultural bias in epigraphic commemoration and burial practices, differential rates of skeletal preservation, and the impact of bureaucratic inefficiency and attempts at tax evasion on the quality of census records. As a consequence, modern studies that failed to show appropriate consideration of these obstacles have become widely discredited.[41] At the same time, ancient historians have traditionally been compelled to make the most of inadequate evidence. Little if anything that can be accomplished by ancient historians would stand up to the standards of the social history of the more recent past. Thus, 'a frail tissue of tenuous conjectures' is often the only alternative to the paralysis of honest scepticism.[42]

The opposite position, advocated by Hopkins in the 1960s and adopted more recently by several other historians, is one of rigorous rejection of all those data that cannot fully be trusted to produce reliable or representative results, and of potentially coincidental matches between such data and predictive models. This sceptical approach requires us to abandon attempts to derive vital statistics (such as mortality, fertility and sex ratios) directly from ancient sources, and to fall back on modern demographic models of age distribution and life expectancy which, whilst based on much more recent populations, are thought to provide a better approximation of conditions in antiquity than any sample of primary evidence. The predictions of these models are to be used as an independent framework for

[41] Unfortunately, what is acknowledged in principle is not always observed in practice. Shaw (1982) 24, commenting on Hopkins's work, notes that 'what is most disturbing (. . .) is that even those historians who know of his work and cite it do not seem to have grasped some of the fundamental principles he has demonstrated'; he refers to Lassère (1977) 479–596 and Salmon (1974), two obvious candidates. Since then, this problem may have abated but has not entirely disppeared, as shown by the work of Suder (1990) and Sgarlata (1991).

[42] Compare Jones (1948), who gives demographic issues pride of place in his survey of ancient economic history (3–10; quote: 10).

qualitative population studies. Work informed by this method includes Hopkins's early assessment of epigraphic evidence for the Roman age structure, Saller's reconstructions of the workings of the Roman family against the background of schematic model simulations, and Parkin's pervasive scepticism about the utility of ancient population data. Among Greek historians, Hansen has adopted a similar course by interpreting reported population figures in the context of a model-based framework.[43]

Beginning in the early 1980s, Frier's work has been characterised by an intermediate position, recognising the value and superior quality of models and comparative evidence but simultaneously aiming to salvage ancient source material whenever this appears feasible. This approach has been in evidence in his influential studies of 'Ulpian's life table', skeletal data from Pannonian cemeteries, North African funerary inscriptions, and the census returns and tax lists from Roman Egypt.[44] In theory, modern model life tables are employed as heuristic devices in helping to determine which ancient evidence has some *prima facie* claim to coherence and validity. In practice, some measure of credibility is accorded to such data that can be shown to be consistent with the predictions generated by these models.

In addition to these three idealised operating modes, there have been attempts to invert the guiding principle of the third position by using ancient data to test the plausibility of modern model life tables and their applicability to ancient populations. This minority position has traditionally been associated with paleodemography, the reconstruction of demographic features from skeletal samples.[45] However, owing to the numerous pitfalls in assessing and extrapolating from osteological material, acceptance of the results of these studies usually requires a greater amount of faith and suspension of disbelief than most historians are ready to muster.[46] Nevertheless, it would

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[43] Hopkins (1966); Hansen (1985); Parkin (1992, 2000); Saller (1994).

[44] Frier (1982, 1983, 1992); Bagnall and Frier (1994); Frier (1994, 1997, 1999, 2000). See also Scheidel (1995b), (1996a) 117–32, (1999a).

[45] Weiss (1973) even goes as far as to devise non-standard life tables based on osteological data. For this reason, his models cannot provide an independent standard for the evaluation of skeletal age distributions. Sperduti (1995), applying the Weiss models to skeletal data, unwittingly illustrates the resultant risk of circular reasoning. For an unconvincing non-standard life table derived from ancient Greek bone samples, see Sallares (1991) 109–13.

[46] See below, n. 66.

be unwise to reject out of hand the possibility that standard model life tables may not be as universally applicable to pre-modern populations as it has come to be taken for granted. In this case, the challenge lies in identifying ancient evidence that can be shown to produce non-standard patterns for reasons other than reporting bias.[47]

Mortality, life expectancy and causes of death

The age structure of a population is a function of three variables: mortality, fertility and migration. There is no ancient evidence that permits direct measurement of any of these factors.[48] Therefore, the only way to estimate age-specific mortality rates is to infer them from documented age distributions. In a second step, life expectancy and likely fertility are then extrapolated from the inferred death rates. This procedure does not control for migration or reporting error. As a consequence, only the most glaring or demographically impossible deviations from the patterns predicted by standard model life tables can reasonably be identified as recording biases. Minor variation, by contrast, may or may not have been caused by genuine differences between ancient age patterns and standard models.

The census returns from Roman Egypt are the only type of pertinent source material that reflects the age composition of a living population. These texts, over three hundred of which have so far become known, date from the first three centuries of Roman rule in Egypt. They record the members of individual households with their ages and other specifics. Thanks to their official character and consistently high level of precision, even sceptics have long considered these texts to be the best demographic evidence from the ancient world. First gathered in the 1950s by Hombert and Préaux, in the 1990s

[47] There is a growing body of evidence to this effect from the more recent past: see the following section, and Scheidel (forthcoming a) ch. 2.2. For an attempt to read the urban census returns of Roman Egypt in this way, see ibid., ch. 2.3, and below. Sallares (forthcoming) continues to doubt the universal utility of standard model life tables.

[48] Fewer than a hundred death declarations of the Roman period have survived in Egypt; owing to their small number and various reporting biases, they are not susceptible to demographic analysis: see Scheidel (1999b). The few surviving birth certificates are equally useless. Ages recorded in epitaphs cannot replace proper death statistics; see below, n. 59.

they became the object of a sophisticated demographic analysis by Bagnall and Frier, and further studies continue to appear.[49] In 1997, the original database was expanded by the publication of extracts from additional census documents of somewhat inferior quality.[50]

Any attempt to reconstruct the probable age distribution of the population of Roman Egypt from the census returns is predicated on the assumption that the data preserved in these texts constitute a representative random or near-random sample of all existing age data; that any apparent anomalies can be controlled for; and that gaps in the documentation can be bridged with the help of standard model life tables. Smoothing the attested age distribution by employing moving averages and weighting the data to compensate for the disproportionate share of urban records,[51] Bagnall and Frier establish a credible age distribution from age five onwards which they use to calculate age-specific rates of mortality, which in turn imply certain levels of life expectancy and fertility. The last two steps in particular introduce further uncertainty. Owing to the underrepresentation of very small children in these documents, mean life expectancy at birth (*e*(0)) can only be estimated by extrapolating from mortality at later ages, applying model life tables that are premissed on the questionable notion of a stable ratio between mortality levels in different age cohorts.[52] Furthermore, migration has the potential to distort our impression of the average age structure. In the absence of relevant information in the census returns themselves, Bagnall and Frier address this problem by weighting the urban and rural data, thereby creating a supposedly more representative total, and by attempting to trace migrational flows through changes in the age-specific sex ratio. The procedures are based on the belief that no significant segments of the population were able to avoid registration in the long run, and that movements between town and countryside therefore cancel each other out.

[49] Hombert and Préaux (1952); Bagnall and Frier (1994); Frier (1997); Scheidel (1996e, 1997d), (forthcoming a) ch. 2.

[50] For the new data from P.Oxy 984A, see Bagnall, Frier and Rutherford (1997), with the demographic analysis by Frier (1997). Unlike the other census texts, most of which originate from Middle Egypt (Bagnall and Frier (1994) 8 table 1.2), P.Oxy 984A refers to a city in Upper Egypt, either Ptolemais or Lykopolis.

[51] See Bagnall and Frier (1994) 81–2 for these measures.

[52] See below, n. 76. On fertility levels, see the following section.

Despite all these obstacles, Bagnall and Frier maintain that the census documentation is of sufficiently high quality to convey a rough idea of the underlying demographic regime. Disaggregated along gender lines, these data, for most age groups, can be shown to match the predictions of plausible model life tables for high-mortality populations: the smoothed and weighted data for women are consistent with a mean life expectancy at birth of close to 22.5 years (equivalent to Model West Level 2 Females), while the apparently less trustworthy evidence for men points to a marginally higher level of around 25 years. 'Despite obvious uncertainties, we think it warranted to reconstruct for Roman Egypt female life expectancy at birth of from 20 to 25 years, life expectancy at age 10 of from 34.5 to 37.5 years, an annual female birth rate of about 42 to 54 per thousand, and an annual female death rate of 42 to 49 per thousand.'[53] In view of corroborative evidence from India and China in the early twentieth century, these findings do not lack plausibility.[54] It also deserves notice that the Egyptian material produces considerably more believable results than Farris's study of census documents from early medieval Japan, which in many ways closely resemble the Egyptian texts and have also been subjected to stable population analysis.[55]

[53] Bagnall and Frier (1994) 90. Cf. 109: 'despite many uncertainties, the census returns make it likely that overall Egyptian life expectancy at birth was in the lower twenties, probably between 22 and 25 years'. For the problems with the male data, see 106–09. Their comparanda of choice come from the 'West' family of the model life tables in Coale and Demeny (1983).

[54] Bagnall and Frier (1994) 88, on India and China, using mainly Barclay *et al.* (1976) for rural China (1929/31): note, however, that other samples from China since 1300 invariably suggest somewhat higher averages (gathered in Lee and Wang (1999) 54–5, tables 4.1–2); compare also Zhao (1997) for (implied) levels of *e*(0) of about 27 to 30 years in a (privileged) Chinese clan population from 1 to 1000 CE, corroborated by marginally higher levels from 1000 to 1749. The Indian values may also be too low: see below, n. 80. Compare Scheidel (forthcoming a) ch. 3 for modern Egypt.

[55] Farris (1985), esp. 18–49, not cited by Bagnall and Frier (1994). The Japanese data are more numerous (five samples from 702 to 732 CE with a total of 5,031 individuals, all of them from villages). Although the Japanese ages are more heavily affected by age-rounding than the Egyptian data, both share a number of characteristics including higher levels of precision for male ages, below-parity sex ratios in the villages (attributable to tax evasion by adult men), and underrepresentation of children. However, in the case of the Japanese data, fits with model life tables often fail, and the few seemingly successful matches yield implausible results: the best data set, males from Hanyu, implies an annual growth rate of 2.2 per cent and a birth rate of 57.14 per 1,000 (Farris 44 table 13; cf. 44–7 for discussion of these puzzling figures, which are partly attributed to 'inadequate registration of the

Even so, two problems remain. First of all, separate analysis of all urban and all rural census age data reveals statistically significant divergencies between the respective age structures, and likewise between either of them and model life tables. This invites an alternative reading of the evidence according to which higher attrition rates in the urban population may reflect urban excess mortality whereas the village records for men are vitiated by reporting errors to such an extent as to make them useless for demographic purposes. In this case, the fit between all female data and standard models would be coincidental, and the census data could not be used to establish country-wide averages, except within margins of error much wider than acknowledged by Bagnall and Frier.[56] More generally, it is unclear how closely the demographic conditions in Roman Egypt—if they could indeed be reconstructed from the census returns—resembled those in other parts of the Roman world, or whether the concept of an average or representative Roman age structure is at all meaningful. Again, the findings of Bagnall and Frier are broadly plausible in that average life expectancy at birth (as a purely mathematical mean) cannot have been much lower than the low twenties, and is unlikely to have been much higher (say, well above thirty). However, given that more than half of their census data come from the Fayum, traditionally one of the unhealthiest parts of the country, and considering both the unfavourable climate and the uniquely high population density of Roman Egypt in general, it is very well possible that in terms of wellbeing, conditions in Egypt may have been substandard.[57] If this were so, evidence from other parts of the Roman empire would be essential in determining an overall mean. However, no data of comparable quality exist. Moreover, even if a purely mathematical average life expectancy or age structure could somehow be calculated, it would be of rather limited utility. It would fail to take account of local and regional variation, whose importance is well attested for the more recent past. In seventeenth-century

populace', no doubt correctly). As a result, the calculated values of *e*(0) of 32.5 years for men (and 28.75 for women), though superficially plausible, remain highly dubious.

[56] Scheidel (forthcoming a) ch. 2. Egyptian tax-lists, derived from census returns for adult men, suffer from the same problems of reporting error as the village census returns themselves: see ibid. The discussion of the tax-lists in Bagnall and Frier (1994) 102–3 is misleading.

[57] See Scheidel (forthcoming a) ch. 1 for a discussion of the severe disease environment of Roman (and later) Egypt.

England, for instance, when national life expectancy is estimated to have fluctuated around the mid-thirties, the country contained parishes with rates as high as fifty and as low as twenty years.[58] Variation in causes of death, a much-neglected subject among ancient historians, is a crucial factor. For this reason alone, the lack of reliable mortality data from the temperate zones north of the Mediterranean Sea is particularly unfortunate. Only a substantial number of local samples from different parts of the Roman world would allow a proper assessment of demographic homogeneity and diversity.

Though preserved in large quantities across the empire, neither epitaphs nor bones can fill this gap. Age data in funerary inscriptions are distorted by cultural biases of age, gender and class, and cannot be trusted to mirror the age and sex structure of the underlying population. Following Hopkins's landmark study in 1966, demographic extrapolations from these sources have repeatedly been discredited and despite occasional relapses are now widely judged to be unpromising.[59] In the meantime, however, Frier has launched an attempt to salvage the age records furnished by Roman tombstone inscriptions in North Africa by showing that in the funerary record of that region, the reported incidence of death among teenagers and young adults is consistent with a standard model life table implying mean life expectancy at birth of 22.3 years.[60] But this is only part

[58] Johansson (1994b) 528. See also Wrigley *et al.* (1997) 268–80; Dobson (1997), esp. 224, 495.

[59] Hopkins (1966); Ery (1969); Clauss (1973); Frier (1983) 336–41; Hopkins (1987); Salmon (1987); van der Horst (1991) 73–84; Scheidel (1991/92); Parkin (1992) 5–19; Saller (1994) 15–18; Parkin (2000). For changing patterns of distortion, see Shaw (1991). Ages at death in Egyptian mummy labels are also useless: see the material in Boyaval (1975) and his comments in (1988, 1992). For a more credulous approach, see above, n. 41: Lassère's position is symptomatic of the common notion that scholarly perseverance can somehow overcome serious shortcomings of our sources, and that a 'Third Way' between simple-minded positivism and wholesale rejection is often feasible: 'repoussant tout autant le pessimisme de L. Henry, de K. Hopkins and de K. K. Ery, que la confiance de Beloch et de Szilagyi, je pense . . . que les épitaphes nous fournissent un materiel qu'il faut interpréter avec rigeur et prudence' (Lassère (1977) 523), an attitude that is clearly born of the perceived need to put to good use whatever ancient source material happens to survive. The recent attempt by Paine and Storey (1997) (replicating Russell (1985) 25 table 3) to re-interpret epigraphic age data from the city of Rome as evidence of 'catastrophic mortality' is singularly unconvincing; see Scheidel (forthcoming d).

[60] Frier (2000). The age-specific rates of male mortality from ages 10 to 40 precisely replicate the pattern predicted by Model South Level 2 Males ($e(0)$=22.299, r=0). Total mortality between these ages is also the same as in the model, $_{30}q_{10}$=65.19

of the picture. The female data fall in a markedly different pattern even within this limited age band, and it is impossible to tell whether this reflects genuine differences or reporting bias.[61] The records for men do not allow us to pin down life expectancy at birth with sufficient precision.[62] It is also worrying that Frier is forced to select the 'West' family of the Coale/Demeny model life tables for his analysis of the census returns of Roman Egypt and the 'South' family for Roman Africa in the same period.[63] What could have made the demographic regime of Roman Egypt more 'western' than that of western North Africa?[64] Apparent arbitrariness in the application of corroborative models casts doubt on the predictive value of such comparisons. Since Frier regards the shape of the mortality curve

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in the data and 65.45 in the model. Higher ages are progressively marred by age inflation, which tends to conceal mortality until old age. For an earlier attempt, see Frier (1982) 235–8, more cautiously trying to show that epigraphic age data from Cirta are 'demographically possible' for ages 5 to 45/55 (but see 236–8 n. 58). Durand (1960) 370, on the basis of a smaller and less homogeneous sample, already argued that male ages from 15 to 42 were the only usable segment of the Roman epigraphic evidence.

[61] The female pattern matches Model South Level 2 Females ($e(0)$=22.5) for ages 10 to 20, then deviates increasingly, especially between ages 25 and 40, when implied mortality is much higher even than in Level 1 ($e(0)$=20), and finally drops below the expected rate after age 45. Aggregate attrition from ages 10 to 50 ($40q10$=49.63) favours Model South Level 1 ($40q10$=49.62), not Level 2 ($40q10$=52.84); the shape of the curve is also closer to Level 1, though still clearly different. Frier notes the possibility that this peak may have been caused by high fertility (which is unlikely, given probable levels of maternal mortality: cf. Schofield (1986); but contrast Demand (1994) 207 n. 4). In any event, reporting bias favouring married women is an equally possible explanation.

[62] A rate of $30q10$ of 65.19 is consistent with $e(0)$=22.3 according to Model South (Level 2 Males), but likewise with $e(0)$=27.7 according to Model West (Level 5 Males). However, the shape of the curve for Model West is incompatible with these records: only Model South, which emphasizes mortality in early childhood and old age, is consistent with the North African data, whereas Model West reckons with a much earlier increase in mortality rates at adult ages. However, the crucial segment of the population—small children—is missing from the African sample. Unless we assume *a priori* that the observed fit for ages 10 to 40 is (1) not accidental and (2) implies corresponding levels of mortality at lower and higher ages, $e(0)$ remains unknown.

[63] Model South cannot be applied to the census data as disaggregated by Bagnall and Frier (1994) or Scheidel (forthcoming a) ch. 2; Model West does not at all fit the North African data. Model South life tables are derived from data from Italy, Spain and Portugal (1876–1958): Coale and Demeny (1983) 12.

[64] It is of course true that as I have pointed out above, demographic conditions in Roman Egypt may well have differed from those in regions with lower population density. In this case, however, a uniformitarian view of Roman age structure and life expectancy would be untenable *a priori*.

(i.e., the relationship of age-specific mortality rates to one another)—rather than proportional attrition within a given age bracket—as the decisive criterion for selecting particular models which are then used to estimate mean life expectancy at birth, even minor biases in recording practices have the potential to lead us badly astray. As in the case of the census returns, the possibility of spurious fits looms large.[65]

Osteological evidence poses even greater problems. Persistent difficulties in assessing the age of human skeletal remains beyond puberty and distorting burial practices conspire to undermine modern attempts to derive representative age or sex distributions, and hence mortality and fertility rates, from cemetery populations.[66] The potential impact of migration further exacerbates these difficulties.[67] Because of all this, we have no reason to put much weight on rare matches between samples of paleodemographic data and standard model life tables.[68]

'Ulpian's life table', the only other known source that can be made to suggest age-specific mortality levels in the Roman world, is a schedule for calculating the tax value of annuities preserved in a compilation of Roman laws. According to Frier, this schedule implies levels of adult life expectancy that are broadly consistent with Model

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[65] Cf. Hopkins (1966) 257–8 for an example. It should also be noted that the same method can be reversed, in that deviations of attested mortality structures from model predictions are sometimes explained with reference to particular disease patterns: e.g., Russell (1985) 93–110; Paine and Storey (1997). Such interpretations are even less reliable than those based on 'positive' fits.

[66] Critical assessments are legion; the following are of particular relevance here: Bocquet-Appel and Masset (1982); Buikstra and Konigsberg (1985); Greene, van Gerven and Armelagos (1985); Johansson and Horowitz (1986); Boddington (1987); Walker, Johnson and Lambert (1988); Lanphear (1989); Milner, Humpf and Harpending (1989); Paine (1989); Piontek and Weber (1990); Siven (1991); Jackes (1992); Konigsberg and Frankenberg (1992); Roth (1992); Johansson (1994b); Konigsberg and Frankenberg (1994); Saunders, Herring and Boyce (1995); Bocquet-Appel and Masset (1996); Hoppa and Saunders (1998); Paine and Harpending (1998); Aykroyd *et al.* (1999). Parkin (1992) 41–58 gives an overview for Roman historians; see also Morris (1992) 72–91.

[67] Sperduti (1995), a demographic analysis of Roman bones from Portus, is a choice example: it is hard to imagine a community more exposed by migration than the port of imperial Rome, and thus less suitable for stable population analysis. Monofactorial life tables (reckoning with a single source of supply, births, and a single attrition factor, mortality) require closed systems. But cf. Osborne (1991) for possible levels of mobility even in rural areas.

[68] Frier (1983) discusses some late Roman cemetery populations in Pannonia which are consistent with model life tables at adult ages; see also Pilet and Alduc-Le Bagousse (1987), for comparable data from late antique and early medieval Normandy. However, for every sample that produces an apparent fit there are many more that do not.

West Level 2, pointing to a mean life expectancy at birth of about twenty-one years.[69] However, subsequent critics have continued to express doubt about the value of this text.[70]

At the end of the day, our results are predetermined by our choice of admissable evidence. It is only by analysing the existing data in a highly selective manner, discarding any samples that are incompatible with reasonable model life tables and/or comparative data (which are ultimately the same thing, standard models having been derived from more recent data), that we are able to construct a coherent picture.[71] This consensual confluence—the internal consistency of the impressions conveyed by a privileged sample—works at two levels. On the one hand, similarities between individual sets of data which have been selected according to their goodness of fit with an independent standard of plausibility (in this case, standard model life tables), are not only unsurprising but inevitable. On the other hand, the fact that is actually possible to identify a number of samples that at least superficially meet this standard deserves attention. While each of this apparent matches could easily be dismissed as coincidental when considered individually, together they perform the function of the poles in Hopkins's wigwam simile, unable to stand on their own but capable of mutually supporting one another by converging at some point.[72] In this sense, the confluence between the several types of evidence marshaled by Frier—from the census returns to Ulpian's life table, North African epitaphs and even Pannonian skeletons—is indeed impressive.[73] If taken at face value,

[69] *Digest* 35.2.68 pr., analysed by Frier (1982).

[70] Parkin (1992) 27–41, 75–8, 82–3 has been the most determined sceptic; see also Hopkins (1987) 120–1; Duncan-Jones (1990) 98–100; Saller (1994) 13–15; Golden (2000). Frier (2000) acknowledges these doubts.

[71] In our case, this entails the exclusion of most cemetery populations (of the type discussed by Parkin (1992) 41–58), of the census data from P.Oxy 984A (Frier (1997)), and of Roman epitaphs from outside North Africa.

[72] Hopkins (1978) 19: 'there are several pieces of evidence, each insufficient or untrustworthy, which seem collectively to affirm [a claim]. I call this the wigwam argument: each pole would fall down by itself, but together thepoles stand up, by leaning on each other; they point roughly in the same direction, and circumscribe "truth"'. Cf. the critique by Shaw (1982) 22–3, reminding us that 'one does not obtain "truth" even in quotation marks by this method'.

[73] 'Although obstacles to research in Roman demography remain formidable (. . .), convergence of the best available statistics still demands a measure of respect': Bagnall and Frier (1994) 109. For further corroborative evidence, see Scheidel (1999a): below, n. 111.

it 'broadly supports the modern belief that average life expectancy of Romans at birth was normally about twenty-five years, or perhaps even slightly lower'.[74] As a result, we may be justified in employing Model West Level 3, implying a mean life expectancy at birth of about twenty-four years for both sexes combined, for the purpose of schematic assessments from fertility rates to family structure. As long as we focus on generalising abstractions of this kind, actual variation over space and time may be put aside. However, the possibility that one or more of the observed fits between evidence and models may be accidental casts serious doubt on the true extent and significance of apparent convergencies.[75]

Further doubt arises from the application to ancient populations of demographic models devised from much more recent statistical evidence. This uniformitarian approach sits uneasily with the more sceptical perspective of the creators of the most commonly used family of model life tables.[76] Two issues require critical consideration. These models are predicated on the assumption of a stable relationship between infant mortality (never adequately documented in ancient sources)[77] and mortality later in life. Because none of the standard

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[74] Frier (2000). He also stresses the lack of strict uniformity, which qualifies the force of 'normally'.

[75] See above, at n. 56 and 62.

[76] The uniformitarian position holds that on average, in the long run and broadly speaking, the four families of model life tables devised by Coale and Demeny (1983) can be expected to fit the mortality experience of all past populations, in that the shape of any particular mortality curve in the past fits one of the curves in these models: e.g., Howell (1976, 1986). Alternative model life tables, such as United Nations (1982), register only minor deviations from the Coale/Demeny standard. Compare Coale and Demeny (1983) 25: 'We must concede that this use of the four families is far from satisfactory, because there is no strong reason for supposing that the age patterns of mortality exhibited in these four families covers anything like the full range of variability in age patterns in populations under different circumstances. (. . .) The question of what is the pattern of mortality in a population of an underdeveloped area is essentially unresolvable (. . .). By the time a population has reached the stage where age-specific mortality rates can be measured with confidence, the level and age pattern of mortality may have changed, so that the pattern of mortality during the underdeveloped period may never be known.' See also Coale and Trussell (1996).

[77] Cemeteries with high proportions of infants can do little to remedy the situation, despite the hopes of Lassère (1987) 94–6, drawing on Guéry (1985) 315 table 1, who tabulates the age distribution in a cemetery in Sitifis, indicating rates of $1q0=25.5$ per cent and $4q1=21.2$ per cent. The first value is consistent with Model West Level 7 Males ($1q0=24.9$ per cent, $4q1=14.9$ per cent, $e(0)=22.9$), the second with Model West Level 3 Males ($1q0=35.2$ per cent, $4q1=21.5$ per cent, $e(0)=32.5$). The fit with Model South is better: Level 5 Males predicts $1q0=25.1$ per cent,

model life tables is based on population with a mean life expectancy at birth of significantly less than thirty-five years, the very high rates of infant mortality predicted for high-mortality populations are supplied by algorithmic extrapolation.[78] As a result, infant mortality in those models that appear consistent with ancient evidence of adolescent and adult mortality tends to be extremely high, of the order of one-third.[79] However, it is far from obvious that under conditions of very high mortality overall, mortality in infancy and at mature ages are balanced at clearly defined ratios.[80] Unless they are, given adult death rates cannot be taken to translate to corresponding rates of infant mortality; actual life expectancy at birth and hence fertility rates remain unclear. It is true that the margins of error are unlikely to be dramatic. Even so, they are sufficient to distort estimates of mean life expectancy at birth by several years.[81] For this reason

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$4q1=24$ per cent, $e(0)=29.3$; Level 6 Males gives $1q0=23.4$ per cent, $4q1=21.8$ per cent, $e(0)=31.7$. If taken at face value, this would suggest that standard models exaggerate infant mortality and also support Frier's comparison of Model South life tables with epigraphic age data from the same region (see above, n. 60). Unfortunately, the impression of plausibility created by a mortality rate of 41.3 per cent during the first five years of life (and also by the presence of a large number of fetuses and stillborn babies, equivalent to 16.3 per cent of all deaths after birth) is immediately undone by the impossibly high proportion of deaths of 30.4 per cent from ages five to twenty and the impossibly low proportion of 28.3 per cent over twenty. A stable population of this kind would quickly have become extinct. We are forced to conclude that since deaths from ages five to twenty are overrepresented in this sample, the same may likewise be true of ages one to five, and that the ratio between infant mortality and early childhood mortality may not reflect reality. Dedicated infant cemeteries are useless for the purpose of relating infant to adult mortality: cf. below, n. 102.

[78] Coale and Demeny (1983) 5, 12, 24–5; Preston, McDaniel and Grushka (1993) 149, 159 n. 1.

[79] Between 33.4 per cent (Model West Level 2 Females) and 32.3 per cent (Model West Level 4 Males), using the models suggested by Bagnall and Frier for Roman Egypt (above, n. 53). Frier (1982) 245 table 5 suggests an even higher rate of 35.9 per cent, extrapolated from 'Ulpian's life table' (above, n. 69–70). In France in the second half of the eighteenth century, mean infant mortality (27.3 per cent) und mean life expectancy at birth (28.7 years) are broadly consistent in terms of Model West predictions (Livi-Bacci (1991) 70 table 12 with 74 table 14). However, despite some very high local infant mortality rates, large composite samples rarely reach thirty per cent, even if life expectancy at birth is very low.

[80] For an overview of evidence from England, Sweden, China, India and Germany, see Woods (1993), esp. 204–12. The work of Mari Bhat (1989) on India is of particular interest here: standard models overestimate mortality up to age 5; mean life expectancy at birth is between one and two years higher than predicted. See also Smith (1991) 57–9, criticising some inflated estimates of overall mortality in fourteenth-century England resulting from the application of standard model rates, and the high-mortality life tables in Preston, McDaniel and Grushka (1993), with lower infant mortality relative to adult mortality compared to Model West.

[81] This can be illustrated by comparing the values for mean life expectancy at

alone, and even in highly schematic simulations, it is always preferable to reckon with ranges rather than single notional averages.[82]

Ancient historians usually concede that infant mortality is an intractable problem. However, one rarely encounters doubts as to whether standard model life tables cover the whole range of mortality patterns at *mature* ages. As I have pointed out above, these models do not include data from populations subject to very high mortality. Given the uncertainties surrounding any demographic analysis of ancient bones, it would be inadvisable to put trust in alternative model schedules derived from skeletal evidence.[83] Nevertheless,

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birth (*e*(0)) according to Models West, North, East and South when *e*(15) is set at 35 years. Since preceding mortality varies depending on the choice of model, *e*(15)=35 implies rates of *e*(0) of 26.42 for Model West, 23.75 for North, 20.62 for East, and 21.12 for South—a range of 5.8 years. Thus, in a stationary population, the Total Fertility Rate would have to be 28 per cent higher in Model East than in Model West (calculation based on the 1966 set of the Coale/Demeny tables, using MORTPAK-LITE: I am indebted to Bruce Frier for bringing these calculations to my attention (pers. comm., 3 August 1997)). Cf. also Parkin (1992) 83–4, 145–6 tables 7–8. However, some historical evidence provides more impressive examples. Wrigley *et al.* (1997) 284 find that in England in the 1680s, mortality up to age 15 corresponded to Level 8 North (implying *e*(0)=36.0) while adult mortality was closest to Level 2 North (implying *e*(0)=21.2); however, they regard this period as a time of transition. Even so, at the end of this transition, mortality up to age 15 resembled Level 11 (implying *e*(0)=43.4), later mortality Level 9 (implying *e*(0)=38.4). What matters here is that owing to lower levels of pre-adult mortality, actual *e*(0) was much higher than implied by adult mortality alone. According to Benedictow (1993) 26 (reported by Johansson (1994b) 528), the same level of *e*(20) can be empirically linked to infant mortality rates ranging from 15 to 35 per cent. The most glaring discrepancies between early and adult mortality rates are attested for China; see Lee, Wang and Campbell (1994) 398 fig. 1, 401 fig. 4: among males in the nobility of Beijing in 1700, $1q0$ was 12.5 per cent (~Model West Level 14 Males, *e*(0)=49.5) while *e*(0) was 22 years (~Model West Level 3 Males), due to very high endemic smallpox mortality from ages 1 to 5 and Model West Level 1 mortality rates after age 10. In 1800, *after* successful smallpox eradication in this group, $1q0$ was 10 per cent (~Model West Level 16 Males, *e*(0)=54.1) but *e*(0) only 36 years (~Model West Level 8 Males), and mortality from age 30 was still as high as in Model West Level 1 Males. The high quality of these data rules out significant reporting errors. See also Lee and Campbell (1997) 63 table 4.2 for distortions in a Chinese non-elite population.

[82] The Roman kinship simulations in Saller (1994) 43–69 are a case in point. Survivorship rates are based on Model West Levels 3 (*e*(0)=25 for females) and 6 (32.5). However, even if adult mortality in Roman society had indeed been consistent with Level 3, we could not be sure whether *e*(0) was closer to the rate predicted by Level 3 or by Level 6. Thus, these two data sets cannot be seen as alternative options, only as markers of a band of probability.

[83] The best-known attempt are the life tables in Weiss (1973), based on fifty samples including skeletal remains from Australopithecines to medieval Europe and population counts of some aboriginal groups. The resultant models suggest very high attrition rates among adults, probably as the result of underageing of adult bones; arbitrary corrections (Weiss 59) fail to solve this problem. For a more recent example,

cemetery populations certainly have the potential of casting doubt on the universal applicability of standard models: in a large sample of crania from medieval Nubia, porotic hyperostosis (bone lesions caused by malnutrition and parasitism in childhood) is associated with a pattern of sub-adult mortality that is very different from standard model predictions, while skeletons that are free from these symptoms fall into a pattern that is not.[84] This raises the possibility that human populations which unlike those considered by Coale and Demeny are exposed to severe environmental stress may experience age-specific mortality rates that are inconsistent with standard models. In any event, this is demonstrably true of a number of specific disease environments: populations that are heavily affected by endemic malaria, tuberculosis or smallpox cannot be expected to conform to the predictions of conventional model life tables.[85] Broadly speaking, demographic statistics of pre-industrial populations produce mixed results: while some data sets are highly consistent with model life tables even at high levels of mortality, others suggest noticeable deviations.[86]

In view of all this, the possible margins of error in applying standard models to ancient populations may well be considerable. At the very least, they make it necessary to operate with ranges instead of any single mortality schedule. This casts doubt on the practical value of further studies of ancient evidence: the most plausible range—20 < *e*(0) < 30 years—has been widely accepted on *a priori* grounds since Hopkins's work in the 1960s.[87] Subsequent evaluation of ancient

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see Sallares (1991) 110, a life table derived from osteological data from ancient Greece (*c.* 650–350 BCE): Woods (1993) 216 n. 33 points out that in this model, adult mortality is far too high to permit survival in the long run. Sallares himself points out that the raw data probably underestimate adult age of death (113).

[84] Mittler and van Gerven (1996), esp. 289–91.

[85] For malaria, see Scheidel (forthcoming a) ch. 2 for further discussion, drawing on Arlacchi (1983), Del Panta (1989) and Dobson (1997); and in greater detail Sallares (forthcoming). For tuberculosis, see Coale and Demeny (1983) 11–12, noting they excluded from their life tables all modern populations with a high prevalence of tuberculosis because this disease had changed the mortality distribution between ages 5 and 40. Cf. Barclay *et al.* (1976) 621–4, on the possible impact of tuberculosis on Chinese age distributions *c.* 1930. The enormous impact of smallpox on mortality between ages 1 and 5 is documented by Lee, Wang and Campbell (1994) 398 fig. 1, 401 fig. 4, 402–3.

[86] Woods (1993) 216 is confident about the relevance of standard models for pre-industrial northwestern Europe but more sceptical concerning South and East Asia. On the other hand, Zhao (1997) demonstrates an impressive fit between data from China (1–1749 CE) and Model East life tables. For more detailed discussion, see Scheidel (forthcoming a) ch. 2.

[87] Hopkins (1966) 264, re-iterated by Scheidel (forthcoming a) ch. 2.

data cannot be shown to have taken us beyond this generic default position. However, while greater precision may often be unnecessary, it is essential for appraisals of ancient fertility rates and the potential for responding to mortality crises.[88]

Unlike mortality rates, causes of death have thus far received very little attention by Roman historians.[89] Demography and disease are usually studied separately. As a consequence, attempts to elucidate demographic conditions regularly fail to progress from description to explanation. The lack of comparative studies of ancient and modern population history is another reason for this unsatisfactory state of affairs.[90] In the absence of medical statistics from the Roman period, we have to rely on proxy data. Records of seasonal mortality provide valuable information: because infectious diseases are responsible for the majority of deaths in high-mortality populations, and the incidence of many major infections varies considerably depending on the season, seasonality patterns may reflect the underlying disease environment.[91] Pertinent evidence is available in the form of dates of death in epitaphs, primarily on early Christian tombstones but also on some pre-Christian grave markers and Egyptian mummy labels. On a number of occasions, these records have been gathered and compared with corresponding statistics from the more recent past.[92] The single most substantial sample, drawn from early Christian

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[88] Stressed by Johansson (1994b) 529.

[89] See Alter and Carmichael (1996); Alter and Carmichael, eds. (1997), on the study of causes of death in historical populations. Dobson (1997), on early modern England, is a landmark contribution. The agnosticism of Parkin (1992) 93 is symptomatic of the perspective of ancient historians. Corvisier (1985) 145–55 discusses age- and sex-specific disease, not causes of death, in ancient Greece. For further thoughts, see Sallares (1991) 221–93; and cf. also Salmon (1992).

[90] In this regard, not even Grmek (1989), easily the most sophisticated study of ancient medical and physical evidence for ancient disease, is an exception. Sallares (forthcoming) and Scheidel (forthcoming a) aim to change this situation.

[91] Alter and Carmichael (1996) 47; cf. Landers and Mouzas (1988) 59.

[92] Nordberg (1963) 55–7 (2,125 data from late antique Rome); Boyaval (1975, 1980) (Roman Egypt); Lassère (1977) 554–6 (Roman North Africa); Patlagean (1977) 92–4 (late antique Palestine); Wietheger (1992) 154–5, 514 fig. 4 (Coptic Egypt); Scheidel (1994a) (Rome), (1996a) 139–63 (Rome, Egypt and North Africa); Shaw (1996) 115–131 (3,938 data from Rome, plus samples from Egypt, Italy, Spain, Gaul, Germany and North Africa); Scheidel (forthcoming a) ch. 1 (1,376 data from Roman and Coptic Egypt and Nubia). Cf. Guéry (1985) 316–9 for an ingenious but unpersuasive attempt to infer seasonal mortality variation from a skeletal sample, based on the observation that the alignment of corpses which were buried pointing to the sunrise varies with the seasons and might therefore enable us to calculate the month of burial: however, his finding that around ninety per cent of

epitaphs in the catacombs of Rome, documents high amplitudes of mortality variation with a strong concentration of deaths in late summer and early fall in most age groups.[93] I have argued that his pattern is best understood as evidence of endemic falciparian malaria and its interaction with other seasonal infections. This interpretation has been corroborated by Sallares's findings on the history of malaria in the city of Rome in antiquity and in later periods.[94] For a long time, no comprehensive study of the second largest regional body of material, from Roman and early medieval Egypt, was available, and more selective discussions were marred by the misinterpretation of some of the texts.[95] The first full analysis of the Egyptian data highlights significant differences between ancient and early twentieth-century Egypt.[96] The implication that non-crisis mortality may have undergone significant changes prior to the modern epidemiological transition will be of interest to historical demographers in general. A handful of smaller regional samples have also produced valuable results, including seasonality data from late antique Palestine which resemble the Egyptian evidence, and data from southern Italy similar both to those from that region in the modern period and from late antique Rome.[97] By contrast, epigraphic material from western Europe is far less useful:[98] however, seemingly random patterns may simply be the result of the conflation of evidence from ecologically and thus epidemiologically distinct locales within larger regions. In

all interments in a cemetery in Sitifis (North Africa) took place from September to March cannot possibly be correct.

[93] Shaw (1996) 118–21 with tables 9, 11–12.

[94] Scheidel (1994a), (1996a) 149–53, (forthcoming d). For malaria in the city of Rome, see Sallares (forthcoming), refuting the scepticism of Shaw (1996) 133 n. 106.

[95] For previous studies, see above, n. 92. Unspecified dates reported on wooden mummy labels have regularly been read as dates of death, in analogy to tombstone inscriptions: e.g., Boyaval (1975) 62, (1980) 282, followed by Scheidel (1996a) 153–5 and Shaw (1996) 121–3. Scheidel (1998) shows that dates on mummy labels refer to the completion of embalmment some seventy days after death. The seasonal distribution of deaths on death certificates (cf. Shaw (1996) 122–3) does not reflect the actual mortality pattern but is heavily affected by reporting biases: Scheidel (1999b).

[96] Scheidel (forthcoming a) ch. 1.

[97] Patlagean (1977) 92–4 (Palestine); Shaw (1996) 126 and 127–8 tables 19, 20–1 (southern Italy).

[98] Shaw (1996) 127–31. His view that the data from Gaul/Germany are 'reasonably informative' (129) does not stand up to scrutiny: cf. the comparative evidence in Wrigley and Schofield (1989) 297 table 8.4. His pattern for Roman Spain, 'discounted as unreliable', looks like the Dutch pattern in the late nineteenth century, which cannot be correct (ibid.). The data from Carthage, which fall into a curiously a-seasonal pattern, are likewise difficult to explain: Scheidel (1996a) 157–63.

that case, they might reinforce the notion of strong local variation in demographic conditions. Age-specific analysis of seasonal mortality is only rarely possible.[99]

Given their various limitations and shortcomings, these data support only tentative conclusions concerning the underlying causes of disease and death. Their main function may lie in illustrating continuity and change—the former in Italy, the latter in Egypt. Moreover, these records reveal the considerable scale of seasonal mortality variation among teenagers and young and middle-aged adults, who dominate the epigraphic record and whose susceptibility to lethal infections remained unmatched in the more recent past. This pervasive feature of Roman seasonality data hints at very high levels of mortality even at mature ages, and once again raises the possibility of significant differences between the mortality schedules of ancient populations and the base populations of standard model life tables, which did not share this particular characteristic.[100] Seasonality data also afford us a unique opportunity to glimpse local and regional variation that would otherwise remain hidden from our view.

Less indirect methods of ascertaining causes of death are less suitable for the assessment of general, representative trends. Infectious diseases would often kill quickly, without causing bone lesions or other telling signs in the osteological record.[101] Soft tissue, above all in mummified corpses, holds greater promise: in the near future, biomolecular analysis is bound to expand our knowledge of disease and causes of death.[102] Finally, the attested incidence of brother-sister marriage in parts of Roman Egypt permits us to estimate probable rates of inbreeding depression, expressed in lethal or debilitating congenital disorders: however, corroborative physical evidence is still lacking.[103]

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[99] In three samples, seasonality may be correlated with age: see Shaw (1996) 118–21 (Rome); Scheidel (1996a) 158 fig. 4.25 (Altava in Mauretania Caesariensis); Scheidel (forthcoming a) ch. 1 (Terenouthis in Lower Egypt).

[100] Seasonality amplitudes do not allow us to infer mortality rates. However, as the scale of mortality variation narrows with decreasing mortality, high amplitudes are suggestive of high mortality overall: Scheidel (forthcoming a) ch. 1.

[101] For this reason, paleopathology is mostly concerned with chronic diseases: Aufderheide and Rodríguez-Martín (1998) give a wide-ranging overview.

[102] To cite a recent example, biomolecular analysis of bones from a late Roman infant cemetery near the Tiber undertaken at UMIST (Manchester) offered support to the hypothesis of the excavators that the children had been the victims of a malaria epidemic: Soren, Fenton and Birkby (1995) 35–9; Soren and Soren (1995); R. Sallares (pers. comm.).

[103] Scheidel (1996a) 9–51, esp. 15–38 (see also (1997a)), in the absence of

Differential mortality is a related issue. In view of comparatively high levels of urbanisation in many parts of the Roman world, especially in Italy and Egypt (see below), differences between urban and rural life expectancies would be of particular interest. In the apparent absence of quantifiable primary data, Roman historians have been reduced to speculating about the likely weight of the 'urban penalty'.[104] In his study of the demographic, social and economic relationship between Rome and Italy, Morley applied Wrigley's estimates of excess mortality in early modern London to Roman capital, calculating the likely volume of migration to Rome accordingly.[105] Drawing on the debate over the extent of urban excess mortality in pre-industrial Europe, Lo Cascio, in this volume and elsewhere, has challenged his claims, and less specific criticism has also been voiced.[106] Even so, the balance of ancient and comparative evidence favours Morley's model of high metropolitan mortality and strong immigration.[107] Outside Rome itself, we can only guess at the demographic pull of the other urban communities of Roman Italy.[108] Most recently, I have tried to show that the urban census returns—mostly originating from the large district capitals of Middle Egypt—suggest higher mortality at mature ages than predicted by standard model life tables.[109] Future reactions will decide whether these data can be accepted as primary evidence of urban excess mortality in the Roman period.

Demographic differences between social classes or occupational groups are similarly difficult to address. Comparative evidence indicates that exposure to infection is a more important determinant of

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supporting primary evidence greeted with scepticism by Bagnall (1997b) and Straus (1999). Biomolecular analyses of skeletal material from the Roman Fayum are a desideratum.

[104] For qualitative appraisals of the city of Rome as a 'population sink', see, e.g., Sallares (1991) 89; Hopkins (1995/96) 60.

[105] Morley (1996) 33–54, drawing on Wrigley (1967).

[106] Lo Cascio (2000a, forthcoming c), and in this volume. For the broader debate, see Sharlin (1978, 1981); Finlay (1981); Perrenoud (1982); van der Woude (1982); De Vries (1984) 175–98; Woods (1989); Dupâquier (1990) and the other contributions in *Annales de Démographie Historique 1990*: 5–151; Galley (1995). On a more general level, Laurence (1997) takes issue with Scobie's striking portrayal of Rome as an unhygienic and disease-ridden dystopia (Scobie (1986)).

[107] Scheidel (forthcoming d) reckons with even higher mortality levels at Rome than Morley, rebutting the views of Lo Cascio and Laurence. Sallares (forthcoming) argues in a similar vein.

[108] Cf. Jongman (1990).

[109] Scheidel (forthcoming a) ch. 2. See above.

life expectancy than access to material resources.[110] Thus, members of the Roman central and local elites who spent much of their time in large conurbations (or army camps) did not necessarily live longer than poor peasants in areas of low population density. My own study of mortality rates among Roman emperors, senators and city councillors has produced results that are consistent, for mature ages, with standard model life tables suggesting a mean life expectancy at birth of approximately twenty-five to thirty years.[111] As usual, the available evidence is far from perfect but meets Frier's standard of confluence in that the various samples generate mutually consistent estimates. On the basis of these sources, it seems that if there really was any difference in life expectancy between the upper classes and the bulk of the population, it must have been small and insignificant. At any rate, the causes of disease and death among the Roman elite—the only segment of the population which is sufficiently well documented in this regard—clearly merit more detailed investigation.[112]

If the rich cannot necessarily be expected to have outlived others, the poor need not have been significantly disadvantaged either. The common assumption that Roman slaves were particularly short-lived may seem intuitively plausible but remains impossible to substantiate.[113] Records of the discharge of soldiers during the Principate have been taken to indicate that among the frontier legions, peacetime mortality was not dramatically different from conditions elsewhere, whereas military units stationed in the city of Rome suffered from

[110] E.g., Livi Bacci (1991) 63–7; Johansson (1994a) 113–4.

[111] Scheidel (1999a) 255–66: *e*(0) is 26.3 years in a sample of thirty emperors who appear to have died of natural causes; approximately 27.5 years for seventeen women of imperial families; about 25 to 31 years for senators; and up to 29.2 years for the *decuriones* of Canusium. For the last sample, see also Duncan-Jones (1990) 93–6 (calculating *e*(0) as 27 or rather 31.7 years), critiqued by Scheidel 264–5. For senators, cf. already Hopkins (1983a) 146–9.

[112] For a first attempt, I can now refer to an unpublished manuscript by Arjan Zuiderhoek (Groningen), 'Mortality, disease and social class in the Roman empire', and his ongoing work on this subject. The biographies of the emperors and collections of letters from Cicero to Symmachus provide large quantities of relevant information.

[113] See most recently Harris (1999) 71, considering a mean life expectancy at birth of twenty years an 'improbably high figure' (!) for Roman slaves in general, necessarily without supporting evidence (*pace* Duncan-Jones (1990) 100–1). However, singularly unfavourable environmental conditions are required to produce this effect, and are unlikely to have prevailed in Mediterranean slave populations as a whole: compare John (1988) 111–3, on extreme levels of slave mortality in Trinidad in the early nineteenth century. While work in mines *vel sim.* must have reduced life expectancy, most Roman slaves worked in domestic or agricultural settings.

exceptionally high death rates.[114] If these discrepancies are genuine, they may reflect the special hazards of the metropolitan disease environment.[115]

Gender bias in survival rates, arguably the most interesting facet of differential mortality, remains completely unknown. In view of the predictive value of female excess mortality for the question of the status of women in general, this lack of information is particularly deplorable.[116] An attempt to derive sex-specific mortality rates from the census returns of Roman Egypt has been unsuccessful.[117] Calculations from skeletal evidence are vitiated by the problems mentioned above. Reliable proxy data, above all on the sex ratio, are also missing.[118]

Finally, mortality crises. The history of epidemics in the Roman world still awaits its first full-scale treatment.[119] There can be little doubt that epidemic outbreaks of infectious disease were common, especially in densely populated locales such as the city of Rome.[120] In most cases, the precise nature of these diseases are impossible to

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[114] Scheidel (1996a) 117–29, superseding the more narrowly focussed discussion in (1995b). See also Alston (1995) 44–7. I note in passing that in this area, errors in calculating recruitment requirements from unit strength and length of service are rife: for references and discussion, see Scheidel (1996a) 121–2 n. 80, 126–7 n. 95.

[115] Scheidel (1996a) 128–9 and n. 107. See above, n. 104–7.

[116] The literature on sex-specific mortality and the status of women in developing countries is very substantial; I confine myself to three references, namely Harriss (1989), with exceptionally rich bibliography; Basu (1992); and Rousham (1999) for more recent references. The underlying causes range from femicide to differential neglect; our understanding of the former, frequently attested in ancient sources, would greatly benefit from quantifiable data: see below, n. 177.

[117] Bagnall and Frier (1994) 106–9, for doubts about their finding of somewhat higher life expectancy for men than for women, at least in part a function of reporting bias; for further criticism, see Scheidel (forthcoming a) ch. 2.

[118] The Egyptian census returns remain disappointing, not least because of their relatively small number: for discussion, see Bagnall and Frier (1994) 92–9, 161–4 (undermined by their own doubts about the male age pattern: see above, n. 53); cf. also Frier (1997) 103–4. Compare Scheidel (forthcoming a) ch. 2 for criticism, superseding my discussion in (1996e) 34–48. Other sources are also unhelpful: ibid., 44 n. 54. For further scepticism, see Parkin (1992) 98–105. On the problems of osteological sex ratios, see Morris (1992) 81–90; and cf. Grauer and Stuart-Macadam, eds. (1998), for attempts to examine gender-differences in health using bones. Most of the scholarly debate on ancient sex ratios has focussed on Greek and Hellenistic history: see below, n. 177, and Guttentag and Secord (1983) 37–52. I will address this issue in a future publication.

[119] The most substantial study of ancient Greek epidemics, Horstmanshoff (1989), is exclusively concerned with literary representations of reactions to such events.

[120] Duncan-Jones (1996) 109–11 gathers pertinent references.

determine, and fatality rates are usually unknown. Academic interest has focussed on the empire-wide pandemic that erupted in the mid-160s and seems to have continued into the 180s CE. Commonly identified as smallpox, this event is now once again credited with severe disruptions of the rythms of life.[121] In particular, Duncan-Jones's detailed study of various categories of proxy data has shed new light on the scale of this disaster in different parts of the empire.[122] Possible medium-term repercussions for the Roman economy have also begun to be discussed.[123] While it is attractive to interpret census documents reporting local drops in population size during those years as indirect statistics of the epidemic death toll, other factors such as flight may also have played a role.[124]

The nature of the widespread epidemic of the 250s and 260s CE remains obscure.[125] By contrast, the emergence of epidemic bubonic plague in the 540s CE has attracted a fair amount of attention.[126] In this case, sources in Near Eastern languages and shifts in the archaeological record help expand the scope of our investigations. Even so, the scale and long-term impact of the plague mortality is hard to establish, and comparative material cannot be used to support any single scenario.[127]

A general study of Roman epidemics would ideally be modelled on Garnsey's account of ancient famines and food crises,[128] situating attested events on a scale from minor outbreaks to global pandemics.

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[121] Boak (1959); Littman and Littman (1973); Salmon (1977) 133–9; Duncan-Jones (1996). An earlier attempt to belittle the impact of the epidemic (Gilliam (1961)) must now be judged unpersuasive.

[122] Duncan-Jones (1996) 115–36, esp. 120–36, discussing Egyptian village population figures; changes in the character of Egyptian land leases; the chronological distribution of army diplomas, dated documents in Egypt, dated inscriptions in Rome and Italy, and public buildings in Roma and Italy; changes in the volume of brick production, marble quarrying in Phrygia, and coin output in Rome and Egypt. All these independent samples reflect a major slump during the plague years, indicating 'that the historians' indications of a major widespread catastrophe are largely correct' (136). For regional variation, see now Ehmig (1998).

[123] E.g., Rathbone (1997) 215–6; Sharp (1999) 185–9. See below, n. 301.

[124] Casanova (1984, 1988); Savorelli (1989); Rathbone (1990) 114–9; Duncan-Jones (1996) 120–1.

[125] Zinsser (1935) 138–41; Salmon (1977) 140.

[126] Russell (1968); Biraben and Le Goff (1969); Dols (1974); Biraben (1975) 25–48; Patlagean (1977) 87–91; Allen (1979); Bratton (1981); Conrad (1981); Biraben (1989); Durliat (1989); Congourdeau (1993); Conrad (1994). On earlier evidence of plague in classical antiquity, see Thüry (1977); Marasco (1998) 46–7 (misguided).

[127] See Alston's paper in this volume.

[128] Garnsey (1988), with Scheidel in Garnsey (1998) 291–2.

More detailed comparative appraisals of ancient Mediterranean and Chinese epidemics, expanding on McNeill's work, are also necessary.[129]

Nuptiality, fertility and fertility control

Roman fertility rates are nowhere directly attested.[130] They need to be estimated from likely levels of mortality, based on the premise that on average and in the long run, the relationship between mortality and fertility was highly inelastic.[131] For this reason, modern estimates of Roman fertility are a function of modern estimates of mean life expectancy at birth derived from matches between ancient distributions and model life tables, as discussed in the previous section. Under these circumstances, rates of growth and population change can only be assumed, never documented.

Fertility, especially marital fertility, is determined by a variety of preventive checks, from age of (female) first marriage and the incidence of divorce and remarriage to contraceptive behaviour, such as abstinence, coitus interruptus, breastfeeding and chemical contraception.[132] In cultural if not biological terms, abortion and infanticide can also be classified as preventive measures.

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[129] McNeill (1977) ch. 3.

[130] Attempts to calculate total fertility (as opposed to the number of children surviving beyond a certain age) from attested parent-child ratios in epigraphic and literary sources are signally misguided: see, e.g., Lassère (1977) 494 for the claim that fertility can be deduced from the number of surviving children recorded in epitaphs. As his own data show (495), the mean number of surviving children in thirty-six North African communities varies from 1.3 to 6.0 per woman (!), or from 1.3 to 2.9 if samples with fewer than ten families are excluded; as a result, the overall mean of 2.25 children/couple is of dubious relevance. For another and dishearteningly recent example, see Suder (1999), reporting 1.32 children per senatorial woman in the first two centuries CE, a finding immediately invalidated by Suder's own overview of analogous tallies from other samples (from senatorial and imperial families from the first to the fourth centuries CE, ranging from 1.5 to 1.6, 1.8 and all the way to 3.9 children). This alone makes it clear that the ratio between attested and actual children varies and cannot be known; see Scheidel (1999a) 278–9, for the scale of the problem. It seems similarly impossible to determine mean fertility rates from parturition marks on female skeletons (see Riddle (1997) 20 table 3 for a compilation of data: below, n. 160). I can think of no other potential evidence from antiquity.

[131] See Bagnall and Frier (1994) 84–9 for constraints on estimates of long-term growth rates.

[132] Fertility differences between monogamous and polygamous unions, while important in principle, are of no relevance for Roman history.

The question of mean age of first marriage has repeatedly been addressed through the medium of Latin epitaphs. In the case of the relatively small number of texts recording both age at death and length of marriage, the age of marriage can be established by a simple subtraction, allowing for some distortion caused by age-rounding. This limited database has been very considerably expanded by the statistical analysis of commemoration practices: because unmarried women are more likely to be commemorated by their parents, the incidence of marriage at different ages can be gauged from the prevalence of different types of dedicators. With growing samples and increasing sophistication, estimates of the mean age of female first marriage in the Latin half of the Roman empire have gradually shifted from the mid-teens through the late teens to close to twenty years: the average bride was about ten years younger than the average groom.[133] The census returns from Roman Egypt have yielded similar results, with a mean age for women of close to twenty years and a marginally smaller age gap between spouses.[134] All these sources point to near-universal marriage for women. Together with a relatively low age of first marriage, this translates to a strong potential for high fertility. At the same time, the low levels of remarriage of fecund women beyond age thirty indicated by the Egyptian data must have had a dampening effect on total fertility.[135] It has yet to be determined to what extent this was also true of other parts of the empire.[136] Shaw's quantitative assessment of the seasonal distribution

[133] The three principal contributions are Hopkins (1964/65) (twelve to fifteen years), Shaw (1987a) (late teens) and Saller (1994) 25–41 (closer to twenty than to fifteen years). See also Carletta (1977) for the Christian evidence. For the male age of marriage, see Saller (1987) and (1994) 25–41 (closer to thirty than to twenty-five). For criticisms of Shaw's method, see Aubin (forthcoming). Age-rounding corrupts calculations of a given interval from a given age: for analogous problems concerning calculations of the recruitment age of Roman soldiers, see Scheidel (1992), (1996a) 97–116.

[134] Bagnall and Frier (1994) 111–21; Frier (1997) 104–8; and see esp. Saller (1994) 68 table 3.4 for a comparison of the western and Egyptian results. In Egypt, the overall mean age gap between spouses is 7.5 years, but 8.3 years in exogamous unions and 5.4 years in close-kin marriages (Bagnall and Frier (1994) 118, 131).

[135] Bagnall and Frier (1994) 153–5.

[136] Saller (1994) 68 claims that there is no empirical evidence from the western empire that would allow us to investigate this issue. This may not be the final answer: in theory, if men were more likely to remarry than women, women would be increasingly less frequently commemorated by spouses as they aged than men (e.g., comparing men over forty (mean age of marriage plus ten years) to women over thirty (mean age of marriage plus ten years)). For qualitative studies, see Bradley (1991) 156–76 (elites); Krause (1994–95) vol. 1, 58–66 (in general). Age in excess of thirty years appears to have been a serious impediment to female remarriage:

of Roman marriage has corrected misleading impressions created by the literary tradition.[137]

In this connection, the age-specific birth rate—the 'shape' of fertility—is the crucial factor, and it is here that recent work has brought a major breakthrough. Frier is undoubtedly correct in pointing out that 'from a demographic standpoint, perhaps the single most important aspect of the Egyptian census returns is that they shed considerable light on fertility, and particularly on marital fertility, among ordinary Egyptians'.[138] As a consequence, his reconstruction of Egyptian fertility rates and his accompanying paper on natural fertility and family limitation in the Roman empire could justifiably be regarded as the single most important contribution to the study of ancient demography to date.[139] The census returns reveal that age-specific fertility (calculated from the ages of women and their surviving co-resident offspring) closely follows a natural fertility pattern. In this scenario, the development of the birth rate with increasing age is determined by fecundity, the physiological capacity to bear children. Most importantly, signs of a stopping strategy—the deliberate cessation of procreation once a certain number and/or sex ratio of children has been reached, causing fertility at higher ages to decline more sharply than dictated by nature—are conspicuous by their absence.[140] In this regard, the population of Roman Egypt resembles early modern European populations prior to the fertility transition. The Egyptian census returns have become the oldest quantifiable evidence of natural fertility in world history.

Frier's inevitably exclusive focus on Roman Egypt raises the already familiar question of general applicability. Since the reproductive strategies of Egyptian couples is consistent with those in other pre-transitional populations, the most economical conclusion is clearly that similar conditions prevailed in other parts of the Roman world as well. Although this cannot strictly speaking be taken for granted, the burden of proof rests squarely on anyone wishing to advocate a different view.[141]

Krause 114–22, and cf. 79–84 for evidence of long-term widowhood. The odds are that in this respect, conditions all over the empire were similar to those in Egypt.

[137] Shaw (1997). For an analogous study of birth seasonality, see Shaw in this volume.

[138] Frier (1997) 108.

[139] Bagnall and Frier (1994) 135–59; Frier (1994), (1997) 108–12.

[140] See above all Frier (1994) 323–7.

[141] But see below, at the end of this section. This is not to say that in other

Even so, this generalising default position leaves ample room for exceptions. Above all, the reproductive behaviour of privileged groups requires special consideration. Roman literary references to family limitation often centre on elite families. The notion of deliberate family limitation in the upper classes seems *a priori* plausible and is fully consistent with comparative evidence for more recent pre-modern aristocracies.[142] In my study of the marital fertility of Roman emperors, I have identified a few rare cases of possible stopping behaviour at the very top of Roman society.[143] While Roman emperors, on average, appear to have reproduced at replacement level, elite fertility in general remains impervious to quantification.[144] In principle, there is no need to evoke reproduction below replacement level in order to account for the ongoing disappearance of old elite families and the emergence of new ones: some families always die out while others expand.[145] We cannot tell whether Roman senatorial or equestrian couples produced fewer (legitimate) children than others, but the mere possibility cannot be dismissed out of hand. It is hard to see how we could ever advance beyond this noncommittal position.[146]

The Egyptian census data document lower levels of fertility outside marriage, mostly for slave women.[147] However, the observation

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respects, the demography of Roman Egypt did not differ from other parts of the empire: see above, n. 57, on mortality, and note the unique incidence of sibling marriage in Egypt (e.g., Scheidel (1995a, 1996b, 1997a)). It also bears mentioning that the Egyptians were said to adopt unusual reproductive strategies by raising all their children: to what extent this claim is simply a facet of the topos of 'fertile Egypt' or marks genuine difference remains unclear; see below, n. 176.

142 See Johansson (1987) for comparative data, and Frier (1994) 332 for ancient claims and further references. See also Livi-Bacci (1986), and cf. Boone and Kessler (1999) 270–5 for a Darwinian perspective.

143 Scheidel (1999a) 274–5, on the families of Marcus Aurelius and Septimius Severus.

144 See Scheidel (1999a) 269–73 for a fit between the number of surviving children of 74 emperors and the family simulations of Saller (1994) 57, 63. Hopkins (1983) 78–107 discusses Roman elite fertility in general. I have not seen Salmon (1999a). Needless to say, extramarital elite fertility is totally unknown (though likely to have been substantial for males): Scheidel (1999a) 279, with Betzig (1992).

145 See Saller (1994) 48–65, esp. 64 table 3.3.e, for likely levels of childlessness in the Roman elite.

146 For the sake of completeness, I should note that lead poisoning as a cause of marital (elite) sterility has finally gone out of fashion, despite attempts at resurrection as late as the 1980s: see Phillips (1984); Scarborough (1984); Needleman and Needleman (1985). Cf. Scheidel (1999a) 274 n. 57.

147 See Bagnall and Frier (1994) 144–5 for the difference between marital and non-marital fertility; the Total Fertility Rate is 6.0, the Total Marital Fertility Rate is 8.4 (ibid. 143 and Frier (1994) 325). It is important to remember that these figures are entirely a function of their estimate of life expectancy, which can be no

that the fertility of slave women is similar to total fertility in the female population raises the possibility (yet fails to prove) that slaves achieved substantial reproductive success outside legally recognised unions, conceivably even at or near replacement level.[148] This finding is not without relevance for our understanding of the evolution of slavery and the slave supply in the Roman world. The problem of unfree fertility in the core areas of Roman slave society, especially in Italy, cannot be solved with the help of ancient sources.[149] My recent argument in favour of relatively high rates of natural reproduction and replacement has been rejected by Harris.[150] In any event, since it does not seem feasible to reckon with very low rates of servile fertility and a very large slave population, we are compelled to choose between these options, or aim for an intermediate position.[151] All we can say with confidence is that the fertility of freed slaves must have been fairly modest.[152] The reproductive behaviour of prostitutes (often themselves slaves) also belongs to the sphere of extramarital fertility. Again, numerical measurements are impossible: the view that members of this group actively sought to avoid pregnancy is supported by literary and comparative evidence and by general considerations of plausibility.[153]

In quantitative terms, exceptional constraints on the fertility of small or marginal groups are far less significant than changes in the

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more than an approximation: see above, at n. 87. However, there can be little doubt that the actual Total Fertility Rate must have been of the order of 5 to 6 children per woman surviving to menopause.

[148] Bagnall and Frier (1994) 158 n. 85, noting that the share of female slaves in all women aged 15 to 50 is very similar to the share of slave children under 15 in all children, which is 'strong evidence that slave women experienced approximately normal female fertility'. Harris (1999) 67 n. 39 fails to see the potential significance of this observation. Nevertheless, it would also be possible to argue that some (few, many?) of these slave children had not been born to slave mothers but exposed or sold after birth (thus Bagnall (1997a)). In that case, total fertility among the free would be higher and slave fertility lower than suggested by the raw data. It is true that in modern slave societies, slave fertility was sensitive to marital arrangements (see, e.g., Scheidel (1997c) 169 for references); the frequency of Roman slave 'marriages' is unknown, despite the assertions of Harris (1999) 65. Ancient textual references to slave fertility are unhelpful: e.g., Scheidel (1994b).

[149] See Bradley (1987) for a balanced discussion of this question.

[150] Scheidel (1997c); Harris (1999).

[151] Scheidel (1999d, forthcoming e). It does not help that both the size and the growth of the Roman slave population are unknown: see below, 'Population size and population change'.

[152] Scheidel (1997c) 167–8. Thus, former slaves could not have made a significant contribution to Italian population growth, contra (e.g.) Lo Cascio (1994b) 114, 116.

[153] E.g., Frier (1994) 331.

fertility regime of the general population. The latter phenomenon has exercised the imagination of ancient historians with bewildering regularity. In this, rather than focussing on the application of preventive checks on overall levels of fertility designed to keep long-term growth at sustainable levels, above all through extended birth spacing, scholars have traditionally equated fertility control with family limitation and associated the latter with demographic contraction: in the most common scenario, family limitation results in below-replacement fertility (*vulgo* 'childlessness') which causes entire populations to shrink and 'decline'. This notion has been as pervasive as it is misconceived. In my view, it must be attributed to three main factors: conceptual confusion, such as the inability to distinguish between fertility control, family limitation, and demographic contraction; unwarranted infatuation with moralising rhetoric and pro-natalist sentiments in ancient sources; and complete ignorance of the growing body of modern research on determinants of fertility and fertility transition theory. In this area, the dissociation of ancient history from the social sciences has proven particularly detrimental to our understanding of ancient populations.

In his fundamental study of modern perceptions of the causes of the disintegration of the western Roman empire, Demandt recalls a sermon at a wedding he attended in Rome in 1980, in which the officiating priest reminded the couple that ancient Rome had prospered as long as the family had been intact but fallen when couples had begun to give in to their individualistic inclinations and refused to have children.[154] This anecdote illustrates very well the interplay between the sympathetic reception of the classical tradition and contemporary ideological contentions and prejudices. Indeed, the association of family limitation and wholesale 'decline', common in ancient literature, is still very much alive. One of the most accomplished historians of the 'Annales' school, Pierre Chaunu, believes that the later Roman empire lost three quarters of its population in some kind of collective suicide of childlessness, something which the 'révolution contraceptive' threatens to do in our own day. For him, both late antiquity and modernity represent examples of a 'civilisation de refus de la vie'.[155]

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[154] Demandt (1984) 577 n. 2.

[155] Chaunu (1981), esp. 232, 272, 275–6, 324. At the beginning (11), he advertises his fear of 'la société de vieillards, sans adultes et sans enfants' (!). By then he

Unfortunately, sweeping claims of this nature are not confined to preachers or historians on a break from serious research. According to John Riddle, Chaunu's dreaded 'contraceptive revolution' had already occurred in antiquity. In two recent monographs, Riddle argues that ancient and medieval women had widespread knowledge of and access to chemical contraceptives which were not only efficient but were actually used frequently enough to be of serious demographic consequence. Thus, in the end, 'there were too few Romans facing too many Germans and Goths, Picts, and Persians'.[156] This line of reasoning rests on a number of logical leaps, from the assumption A that efficient contraceptive agents exist in nature to the assumption B that they were known and could be properly administered to the assumption C that the bulk of the fecund population (i.e., married couples) desired to make use of them to the assumption D that they did so on such a scale that demographic contraction became inevitable.[157] The first two points are of little consequence here.[158] Even if we took them for granted, if only for the sake of argument, the two following premises are completely unsubstantiated and *prima facie* implausible, especially D. No other known pre-transitional population has been shown to conform to this model. Moreover, as mentioned above, the only quantifiable evidence from the Roman period fully supports the natural fertility scenario.[159] In his second book,

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had already devoted a separate book to the subject of 'réfus de la vie' (Chaunu (1975)). It is not without irony that Chaunu is best known for his awesome doctoral thesis in twelve volumes which contains thousands of pages of statistics on trade between Spain and the Americas in the early modern period (Chaunu and Chaunu (1955–59)). Apparently, the study of ancient history is not thought to require comparable rigour.

[156] Riddle (1997) 86. One might have thought that the Goths were Germans, and that the Picts were unlikely to equal more than a small fraction of one per cent of the population of the Roman empire. For the fallacy inherent in this perspective, see below, at n. 273–6. Another comical touch is provided by his rhetorical question, given the existence of so many herbal contraceptives, 'why there is any population in the Mediterranean at all' ((1992) 38).

[157] None of these three separate connections is addressed in Riddle's work. Compare the classic statement by Coale (1973): fertility control must be 'within the calculus of rational choice' for couples; they must want smaller families; and the means must be available.

[158] Cf. van de Walle (1997) for a discussion of the use in the Middle Ages of herbal medication for the purpose of menstrual regulation (i.e., for health reasons) rather than abortion (but cf. Hopkins (1965) 132).

[159] See Frier (1994) 327–33 for a convincing refutation of the Riddle position. King (1998) 132–56 adds an incisive discussion of the nature and ideology of some of the ancient sources on which Riddle's claims are based: see below, n. 169. See now also Frier in this volume.

Riddle not only dismisses this unique source of information but treats his audience to an indigestible cocktail of errors and fantasies that turns the demographic sections of his work into an embarrassing travesty of scholarship.[160] Using epigraphic evidence, Wierschowski has launched a similarly unconvincing attempt to demonstrate a

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[160] At this point, the most egregious instances will have to suffice. Riddle (1997) 17, in order to cast doubt on the (incontrovertible: see above, at n. 87) claim by Bagnall and Frier (1994) that the Total Fertility Rate in Roman Egypt was close to six children, refers to 'other ancient records' which 'albeit scant, support a hypothesis for smaller family size long before the Roman censuses were taken'. This 'other evidence' turns out to be Assyrian statistics reporting 1.43 children per family, corrected to 'between two and three children' to account for unrecorded prebubescent offspring, 'a figure much below Bagnall and Frier's rate for children born in Roman Egyptian families'. He also adduces further comparative data for mean numbers of living children per household in the Middle Ages. Hence, Riddle compares tallies of surviving children with an estimate of all children ever born, which does not make any sense: his claim that Frier operates with 'a much higher fertility rate than that used by most other investigators' is therefore completely void. According to the Egyptian census returns, two-parent couples with coresident offspring reported on average 2.25 children; this rate would be lower if we included childless and one-parent families. To judge from the kinship simulations in Saller (1994) 48 table 3.1.a, under the mortality conditions posited for Roman Egypt, no woman would *on average* have more than 2.3 living children at any age, while the overall mean would be lower. Riddle also tries to link alleged demographic contractions in the Roman and early medieval period (15 fig. 1) to fertility control, allowing that 'wars, famine, climate, pestilence, and exposure of children always explained some results', but maintaining that 'those calculations were not sufficient to explain the few children born, especially in times of seeming health and prosperity' (14). No documentation is provided, and it is hard to see where it could come from. The circular character of a model that seeks to 'explain' invisible phenomena is obvious. Riddle 20–1 uses osteological data indicative of the number of full-term pregnancies of women in the Eastern Mediterranean to trace the development of average fertility rates: the mean drops from 5.0 parturitions in 2000 BCE to 4.7 in 1500 BCE, 4.1 in 1,150 BCE, 3.6 in 300 BCE and 3.3 in 120 CE (20 table 3). In his view, these figures 'imply that fertility rates had fallen below what was necessary to maintain the population just as the Roman empire reached its zenith' (20), a claim apparently based on the assumption that 3.5 children were needed to keep a population stationary (cf. 16–17). Unfortunately, the mean number of births per woman is different from the Total Fertility Rate, which measures mean completed fertility of women surviving to menopause: thus, the average (buried) woman always had significantly fewer children. However, if we decided to take the skeletal data to reflect Total Fertility, and a mean of 3.5 to maintain a stationary population (implying $e(0)$=37.5 according to Model West, clearly too high an average: see above, at n. 87), the population of that region would have grown by close to 1,000,000 per cent between 2000 and 300 BCE. (Riddle also adds data from Herculaneum in 79 CE, a with a mean of 1.81 births per woman, noting that all these women died simultaneously: however, there would be no dramatic difference in age distribution (and hence mean birth rate) between this sample and any skeletal sample unless the Herculaneum sample was vitiated by sampling bias in favour of younger women—were only young women killed by the volcano while the older ones ran or swam to safety?). But even if we abandon Riddle's erroneous association of these data with the Total Fertility Rate and view them as mere indicators of trends, they

significant drop in Roman fertility rates.[161] Once again, a mixture of contraception, abortion and infanticide/exposure is held responsible for this alleged development.[162]

To some extent, modern academics advocating these models may simply have been taken in by the rhetoric of classical authorities. In

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produce incredible results. The implication that the mean birth rate could have dropped by one-third from 2000 BCE to 120 CE is problematic: if in 2000 BCE, net growth was 0.5 per cent per year (an improbably high long-term rate) and *e*(0) was 25 (or 30) years, growth would have dropped to −1.0 per cent by 120 CE (and to −0.5 per cent by *c.* 500 BCE, assuming a steady trend between 1150 BCE and 300 BCE). 620 years at −0.5 per cent growth per year would have reduced the population by 95 per cent; the reduction implied by the rates for 300 BCE and 120 CE would be even higher. It also seems unlikely that mortality could have dropped far enough between 2000 BCE and 120 CE to accommodate the reduction of the birth rate: this would require a shift from, say, Model West Level 2 to Level 7, implying a 55 per cent increase in *e*(0). In other words, there is no way of reconciling the differences between the osteological markers with any feasible long-term growth rates, and we must conclude that these data cannot illuminate changes in population growth. (For a similar problem, see below, n. 161).

161 Wierschowski (1994) 374 table 2, a comparison of the mean number of attested children per family in epitaphs from three Gallic communities in different periods of the Principate. According to this tabulation, this mean changes from 1.59 children per family in the first century CE to 1.23 in the second and 1.24 in the first half of the third century CE. While admitting that the actual number of children remains uncertain, he insists that the observed trend as such 'proves' a decline of the birth rate (375). This raises the same problems as in the case of Riddle's osteological data. If the population in these communities had been stationary during the first century CE (an implausible assumption) and *e*(0) had been as high as in France in the 1750s (~ Model West Level 4 Females *r*=0), the birth rate would have been *c.* 36.36 per 1000 in the first century and 28.24 per 1000 in the following 150 years. In that case, the population would have shrunk by 0.812 per cent per year from 100 to 250 CE, a total reduction by 72 per cent, which is not only incredible but also inconsistent with the archaeological record. If, however, Gaul had been growing in the first century, subsequent losses would have been somewhat less dramatic: an annual growth rate of 0.3 per cent in the first century would imply a rate of −0.579 per cent afterwards, resulting in a contraction by 59 per cent, or a swing from (say) 5 million in 1 CE to 6.7 million in 100 CE and 2.7 million in 250 CE, or half what it had been at the beginning of the early imperial expansion. Thus, the differences implied by these inscriptions cannot faithfully mirror changes in the underlying fertility regime. As a consequence, it is impossible to say to what extent they reflect actual change, and to what extent the reporting of children was subject to changes in commemoration practices. I should note that no such development is visible in proper demographic evidence, i.e. the Egyptian census returns: the catalogue in Bagnall and Frier (1994) 181–312 lists 107 couples with 240 children, a mean of 2.24; the earlier half of this sample (54 couples with 121 children from 11 to 159 CE and the later half (53 couples with 119 children from 159 to 257 CE) produce identical means of 2.241 and 2.245, respectively. For further data, cf. Gallivan and Wilkins (1997) 241.

162 Wierschowski (1994) 378. For a more expansive discussion of these factors, see Wierschowski (1996), focussing on Severan (?) measures against abortion and exposure, which are read as reactions to the demographic contraction documented by the Gallic inscriptions.

practice, serious appraisals of changes in overall fertility must have been well beyond the reach even of well-placed ancient observers: even fairly modest trends would have had implausibly strong repercussions, while absolutely minute and therefore less incredible changes would necessarily have remained invisible.[163] It is also impossible to disentangle moralising claims about depopulation from their background of social ideology and literary convention.[164] To make matters worse, ancient tirades about childlessness have become enmeshed in modern models of the 'decline and fall' of the ancient world.[165]

But this is not merely a question of a naïve reading of ancient texts. More often than not, doomsday scenarios of fertility decline and general disintegration serve very specific purposes. In classical antiquity, fertility control raised the spectre of women's control over their own bodies, was associated with the emergence of corrupting 'luxury', and clashed with the pro-natalist ideology of the warrior-citizen state.[166] Until recently, conservative commentators were quick

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[163] Polybius 36.17 is the *locus classicus*, notorious for the sweeping claim that the Greeks of his time raised only 'maybe one or two' of the children born to them, causing the land to be abandoned and the cities to be emptied. On a literal reading, if all children except one or two had been disposed of after birth, Greece would indeed have been stripped of its population. At $e(0)=25$, the Total Fertility Rate in Greece would have had to average 5.2 in order to keep the population from shrinking; a limitation to one or two children would have translated to an annual rate of decrease of from 2.46 to 3.23 per cent. A rate of 3 per cent halves a population every 23 years. Thus, in Polybius own lifetime, the population of Greece would have plunged by almost 90 per cent. On the other hand, a less incredible attrition rate (while incompatible with Polybius' claim about parity) would have been hard to observe: if the Greek population had contracted by half during the last 350 years BCE, it would have shrunk by a mere 0.2 per cent per year, with a shortfall in the birth rate of 5 per cent (i.e., 38 instead of 40 births per 1,000). A drop of that order of magnitude would have been impossible to discover without elaborate serial statistics. If, as seems more likely, Hellenistic Greece had lost population owing to the emigration of fecund adults, it would have been pointless to blame the fertility regime: cf. already Moreau (1949) 612. In general, see Hopkins (1974) 77: 'Very small changes in the factors affecting fertility would have had significant consequences. It is unnecessary to prove that *many* poor people had fewer children (. . .). A small trend would be sufficient. It is impossible to prove such fine changes from the surviving evidence.' Den Boer (1974) 81 misses the point.

[164] For surveys of pertinent sources, see, e.g., Gallo (1980); Grassl (1982) 56–8. Augustus' marriage legislation is perhaps the best-known example: Mette-Dittmann (1991) 131–86.

[165] See below, n. 167 and n. 271.

[166] As King (1998) 156 sums up her rebuttal of Riddle's claims, in ancient Greece 'the myth of effective plant-based contraceptives may thus be a male expression of a fear that women hold the knowledge which could enable them to control the fertility of the household'. See Salmon (1999b) for some of the usual references linking low birth rates to vain and wanton women, luxury and moral decline.

to adopt a similar stance: it is hardly a coincidence that accounts of Roman fertility decline have repeatedly been spiced with criticisms of modern individualism, female emancipation, the growth of non-western populations, and immigration.[167] At the same time, developments that have long been viewed with suspicion and disapproval may also be presented in a favourable light. Recent work on transition theory has shown that fertility decline and female empowerment often go hand in hand.[168] In this regard, both hostile and friendly observers have a point: yet what appears loathsome to the reactionary becomes admirable to the progressive liberal. Riddle's work is a prime example for the latter, with its celebration of secret female wisdom, gradually suppressed by Patriarchy/Christianity/The Inquisition and subsequently ignored by blinkered male academics.[169]

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[167] Zumpt (1841) traced Roman depopulation to luxury and spoilt couples unwilling to raise children, and warned against parallel developments in his own day, advocating curbs on luxury and freedom. For a Nazi view of Roman depopulation, i.a. because of abortion, see Vogt (1935); the German birth rate rose thirty-nine per cent in the first five years after Hitler's takeover, to the delight of some modern demographers: Moreau-Bisseret (1986) 70–1. Compare the quote from Landry (1945) 31 ('la démographie du temps présent, laquelle présente, en beaucoup des pays, des caractéristiques ayant de l'analogie avec celles du passé lointain que nous évoquons' [i.e., Greece and Rome]) in Salmon (1974) 114; see Finley (1958) 158, and more extensively on Landry's analogies between Roman and modern Demandt (1984) 360–1. This perspective is not limited to the study of ancient demography: see very briefly the noncommittal insider's version in Sauvy (1970) 1163–4 (and cf. in general Keyfitz (1996) 335–6), and in great detail the fascinating (if partisan) indictment of Le Bras (1998) of the links between right-wing extremism and professional historical demography in France. For concerns about depopulation in other countries and periods, see, e.g., Hicks (1978); Teitelbaum and Winter (1985). In the words of the French president Jacques Chirac, then mayor of Paris, 'two dangers stalk French society: social democratization and a demographic slump' (*Libération* 30 October 1984, quoted by Teitelbaum and Winter (1985) 123). It is intriguing to note that Frier (1994) 331 and n. 51 feels the need to qualify his observation that under very high mortality, 'a culture that permitted private choice to prevail over wider societal needs was, in effect a culture of suicide; self-interest ultimately yielded to common interest', with the disclaimer, 'to avoid misunderstanding, it is not that traditional pronatalist cultures can be justified by reference to material conditions, but only that they are partially explained on this basis'. The conservative critic from antiquity to the twentieth century would hold that a culture that 'yielded to common interest' was indeed preferable. Cf. above, n. 155, on Chaunu's apparent belief in the existence of a 'culture of suicide', a notion raised as a purely counterfactual alternative by Frier. For the suicidal implications of below-replacement fertility, see also Coale (1986) 22.

[168] E.g., Caldwell (1982); Handwerker (1986, 1989, 1991); Handwerker, ed. (1986); Mason (1985, 1997b); Federici, Mason and Segner, eds. (1993). Cf. also Mackinnon (1995).

[169] Riddle's story is a romantic tale of wise women obtaining, preserving and transmitting critical information without male interference: having determined the

One wonders whether this novel spin, while better attuned to modern tastes, does more justice to history than the older opposite bias. We would do well to stop discovering the present in the past, all the more so as the imminent contraction of many contemporary western populations might well trigger a resurgence of the classical association of fertility control with decline.

Leaving aside literal readings of classical texts and anachronistic ideological concerns, interpretations of ancient fertility also suffer

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properties of chemical contraceptives through 'countless experiments' (Riddle (1992) 87), they proceeded to pass them on just like cooking recipes (Riddle (1992) 155, also quoted by Frier (1994) 329 n. 38): 'The woman's salad may have been her control over her own life and her family's life, while the men and nonchildbearing women ate from the same bowl and saw it as simply a nourishing, tasty meal course. If the salad was prepared correctly and eaten in the correct amounts, a woman would likely avoid pregnancy.' In the Middle Ages, medical training began to shift to universities and medical practitioners lost touch with the female oral tradition of chemical contraceptive recipes (Riddle (1997) 102); at first this was a gradual process—'many people in the late Middle Ages still knew about contraceptive and abortifacient drugs, but they may have used these drugs less frequently than their ancestors did' (125); but the loss of information gained momentum with the Renaissance, when 'medical practitioners could have learned from their patients, of course, but the intellectual mood of the Renaissance caused them to distrust, even scorn, folk ore' (139), even though 'the knowledge of the ancients could be found on the pharmacists' shelves throughout the sixteenth and seventeenth centuries' (148), purveyors who unlike their (wise but not so clever?) female colleagues 'escaped the accusation of witchcraft, probably because they were cleverly vague about birth control agents. They sold them, but nothing written in their shops said exactly what they did. In one sense, sixteenth-century drugstores resembled modern health food stores, where information and proof about usage are hard to come by' (157). Even so, lay wisdom finally succumbed to the onslaught of a multifarious coalition of 'large numbers of single women, spinsters and widows, the increased intolerance for *maleficia* (harmful magic), an intense unprecedented misogyny, and the conflict between the Protestant and Catho ic churches' (165): as a result, 'there are few modern women who know the antifertility plants in their environment, whereas women in the past did know them. (. . .) Whatever we decide on the mora ity of contraception and abortion, we must recognize that women in the past made deliberate decisions about whether to have children and when to have them' (259). As King (1998) 141 remarks, Riddle's 'identification of contraception as women's concern, fertility as men's interest, has the whiff of anachronism; it is a feminist reading creating Greek women in the image of the post-Pill era, able to exercise—preferably without men's knowledge—their "right to choose".' All this has to be seen in the wider context of the popular image of the healer-witch, which offers 'nostalgic pleasures to anxious urban residents' (Purkiss (1996) 20–2, quoted by King (1998) 264 n. 11). The usual suspects from tortured midwives/witches to the Holy Inquisition are duly paraded by Riddle (1997) 110–9 and esp. 167–9, skilfully deploying in the classical 'there are some who say'—mode a bizarre theory on the supposed demographic impact of the witch-hunts espoused by a pair of German academics (one of whom, G. Heinsohn, is also notorious for his claims that the Sumerians are a modern invention and the Assyrians were identical with the Persians).

from a widespread lack of awareness of the questions, concepts and findings of modern fertility transition theory.[170] Even the most superficial acquaintance with the debate on determinants of fertility might go a long way in reining in the imagination of ancient historians and in checking their more eccentric claims.[171] Roman demography does not exist in a vacuum, and only stands to gain from cross-cultural contextualisation.

In sum, there is no good reason to believe that in the Roman period, the population of the empire in general practised forms of family limitation that led to demographic contraction. However, the view that natural fertility prevailed in most segments of that population invites a word of caution. Quantitative evidence from early modern China, thus far neglected by students of classical antiquity, shows that even a pre-transitional population may accommodate a certain amount of family limitation that modifies the age-specific fertility profile. The demographic regime of China, Taiwan and Japan in the seventeenth and eighteenth centuries is characterised by low levels of marital fertility compared to contemporaneous European populations. In addition to late marriage and wide birth intervals, early stopping—accelerating from the late twenties onwards—served to reduce the birth rate.[172] It deserves especial notice that this pattern can be found both in the aristocracy of Beijing and among Manchurian peasants.[173] In this regard, the Chinese fertility schedule differs significantly from the age distribution of births in Roman Egypt. Is it possible that similar diversity existed within the Roman empire?

In China, family limitation was closely associated with infanticide, especially among newborn girls. Femicide, though sensitive to eco-

[170] The most pertinent recent works include Caldwell (1982); Coale and Watkins, eds. (1986); Chesnais (1992); Gills, Tilly and Levine, eds. (1992); Hirschmann (1994); Greenhalgh, ed. (1995); Kirk (1996); Mason (1997a); Wood (1998); Wilson and Airey (1999). For the intellectual background, see Szreter (1993).

[171] E.g., Bulatao and Lee, eds. (1983); Easterlin and Crimmins (1985); Kertzer and Hogan (1989); van de Kaa (1996); Leete, ed. (1999).

[172] In Liaoning (Manchuria) from 1730–1900, some 60 per cent of couples (nobles and peasants) had stopped procreating by age 40, compared to 20 per cent in Europe: Wang, Lee and Campbell (1995) 391 fig. 1. Lee and Wang (1999) 90: 'The Chinese and European age patterns of stopping are thus fundamentally different. European populations contain few early stoppers and follow an exponential pattern of increase, with a rapidly rising rate after age 35; Chinese populations contain many early stoppers and follow a logistic pattern of increase with a slowly tapering rise.'

[173] Compare Wang, Lee and Campbell (1995), on the Qing nobility, with Lee and Campbell (1997) 83–102, on Liaoning peasants.

nomic pressure, was not so much a desperate last resort in times of crises than an accepted means of long-term fertility control.[174] The same is true of Tokugawa Japan.[175] In classical antiquity, the inhabitants of Egypt were singled out for raising all their children. By implication, Greeks and Romans did not.[176] Thus, as far as the availability of post-natal family limitation is concerned, the core areas of the Greco-Roman world may well have been more akin to East Asia than to early modern Europe. As usual, ancient statistics that would permit us to test this assumption do not exist.[177] As a consequence, we are left with an intractable problem: although the Egyptian fertility schedule and comparative evidence from later periods of Mediterranean history support the notion that natural fertility was common throughout the Roman world, the mere existence of the East Asian model lends a measure of credibility to the notion (which could otherwisc have been no more than free-floating speculation) that the inhabitants of Roman Italy or Greek cities *may* have actively engaged in family limitation, at least in part (but maybe not only) after birth.

However that may be, what is emphatically not implied by the East Asian or any other scenario is that such forms of family limitation in pre-transitional societies would result in demographic contraction. Between 1700 and 1850, when the relevant fertility data were recorded, the Chinese population grew by some 160 per cent. Generally speaking, populations shrink in net terms (i.e., excluding migration) either when mortality rises significantly above conventional levels without an attendant increase in the birth rate (due to

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[174] Lee, Wang and Campbell (1994) (Qing nobility); Lee, Campbell and Tan (1992); Lee and Campbell (1997) 58–82 (Liaoning peasants). Other means of fertility control appear to have included marital abstinence (Wang, Lee and Campbell (1995) 396–9 (Qing nobility); Lee and Campbell (1997) 99 (Liaoning peasants)) and possibly contraception and abortion (Lee and Wang (1999) 91–2).

[175] E.g., Saito (1992); Skinner (1993).

[176] Diodorus 1.80; Strabo 17.2.5. Analogous claims were made about other foreign peoples, such as Britains (Dio 76.12.2), Germans (Tacitus, *Germania* 19.5), Etruscans (Athenaios 12.517e) and Jews (Tacitus, *Historiae* 5.5.3). This confirms that they ought to be understood as statements about these authors' own (i.e., Greek and Roman) culture. For the proverbial nature of Egyptian fertility, see Parkin (1992) 113 and 188 n. 99, 189 n. 101. I have argued in Scheidel (1996e) 48–58 and (1997d) that the number of twins recorded in the Egyptian census returns is inflated by cultural preference.

[177] *Contra* Pomeroy (1983); Brulé (1990, 1992); Bagnall (1997a). The purpose and scale of infanticide in the Greco-Roman world remain obscure: most modern work has centred on the Greeks; for an overview, see Oldenziehl (1987). Boswell (1988) 51–179 and Harris (1994) offer comprehensive discussions of the literary evidence for child exposure in the Roman empire.

a variety of events from war and famine to epidemics and tectonic activity) or when fertility drops significantly below previous levels without a concomitant reduction in mortality. The former was bound to happen from time to time, at least in the short run. By contrast, even if we attribute to the average Roman family the ability and the desire to manage the size of their families—an assumption for which solid evidence is lacking—there is no sign in world history that large-scale demographic contraction caused by a reduction of fertility is anything other than a novel phenomenon of developed countries of the twenty-first century that is of no relevance to our understanding of Roman population history.[178]

Migration

Migration is an essential part of the 'Basic Demographic Equation' or 'Balancing Equation', $P_{t+1} = P_t + B—D + IN—OUT$, where a given population at time $t+1$ is the population at time t plus the number of births (B) between t and $t+1$, less the number of deaths (D), plus the number of immigrants, less the number of emigrants.[179] For the student of Roman demography, however, migration primarily serves the purpose of further messing up existing data samples, most of which are highly inadequate to begin with. The mere possibility of migration undermines attempts to reconstruct age structures and sex ratios from census returns or cemeteries.[180] Except in the case of a few newly-founded colonies, the scale of Roman population transfers is usually impossible to measure.[181] Short-range movements, common in other pre-modern societies, cannot be tracked but nevertheless need to be taken into account.[182] That even the most massive transfers defy quantification injects considerable uncertainty into the study of the development of Roman chattel slavery and of

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[178] For rare examples of localised below-replacement fertility, see briefly Coale (1986) 22 (on parts of nineteenth-century Hungary and France).

[179] Newell (1988) 8.

[180] See above, 'Mortality, life expectancy and causes of death'.

[181] See, e.g., the summary for the years 334–263 BCE in Cornell (1995) 380–3, involving a considerable amount of guesswork. See in general Salmon (1969), for the Republic, and also Wilson (1966); Keppie (1983), for the transition from Republic to empire; Mann (1983), for the Principate.

[182] Thus Osborne (1991), with reference to classical and hellenistic Attica, and a tantalising survey of comparative evidence.

differential growth in different parts of the Roman empire.[183] Estimates of the volume of migration to Roman cities are a function of assumed rates of urban growth and natural decrease: in other words, they can only be derived from the combination of two unknown variables. In order to be at all viable, such estimates need to fit into the framework of more general models. For instance, Brunt is compelled to assess emigration from Roman Italy within the constraints posed by his estimate of the total citizen population.[184] Morley estimates probable rates of migration to the city of Rome and their effect on population growth in Italy drawing on Wrigley's 'simple model' of the impact of migration to London on the demographic development of early modern England.[185] Similar attempts have been made concerning the cities of Roman Italy and Egypt.[186] At the same time, the potential strength of such estimates—their interdependence with other variables—makes them vulnerable to the charge of circularity. However, Lo Cascio's alternative scenario, rejecting conventional assumptions about the number of Romans outside peninsular Italy and the impact of migration on the countryside, inevitably relies on an analogous approach: the numerical values of the variables differ while the method is the same.[187] Speculative calculations of the scale

[183] Horden and Purcell (2000) 377–400 stress the lateral mobility of Mediterranean populations, which makes it often preferable to explain depopulation or repopulation in terms of population transfers. However, their impressionist approach sits uneasily with their sweeping claims; for a particularly ill-chosen example, see 385: between 1608 and 1763, 27,000 people left France for Canada; 'that reassures the Mediterranean historian of earlier periods about comparability. It is not difficult to postulate displacements of this order of magnitude for almost any of the centuries that concern us' (i.e., antiquity and Middle Ages). Not difficult, indeed: the French transfer works out at 173 persons per year in a population that grew from 20 to 26 million during the same period, an annual rate of transfer of 0.00075 per cent—equivalent to the dispatch of two kleruchs from classical Athens every year! More appropriate comparative evidence suggests the feasibility of very massive population movements, perhaps most notably in China: Lee and Wang (1999) 116–8.

[184] Brunt (1987) 159–265. His objective is to refute the view that by 28 BCE, *c.* 560,000 Roman citizens then registered in the census lived outside Italy (Frank (1924) 333, who had to place as many Romans as possible outside peninsular Italy in support of his higher estimate of the total citizen population); Brunt 263 arrives at *c.* 375,000, noting that 'I place no reliance on such precise figures. However, they are coherent with what I regard as the best interpretation of the census figure of 28'. For a similarly circular argument concerning the population of the Cisalpina, see ibid. 166, criticised by Lo Cascio (1994b) 108.

[185] Morley (1996) 43–6, 49–50, 53, following Wrigley (1967).

[186] Italy: Jongman (1990) and (forthcoming); Egypt: Scheidel (forthcoming a) ch. 2.4.

[187] Lo Cascio (1994b) 108–9, on northern Italy, and (2000a, forthcoming c) on migration to the cities, likewise in this volume.

of the influx of slaves into Roman Italy are beset by even more serious problems: in stark contrast to recent advances in the measurement of the modern transatlantic slave trade, the shipments of the Roman period cannot be properly quantified.[188] Our guesses depend on unknown and controversial factors from servile sex ratios to the frequency of manumission and the resultant rates of natural reproduction.[189]

In view of all these obstacles, the lack of systematic studies on Roman migration will occasion scant surprise.[190] Wierschowski's recent epigraphical survey of migration in Roman Gaul appears to be the only significant exception. Using references to *patria* in epitaphs, he traces the movement of individuals who predominantly belonged to a small segment of society, namely the wealthy and traders.[191] In the future, appraisals of migration will benefit from the biomolecular analysis of skeletal remains, which may shed light on geographical provenance.[192] What is more, the genetic makeup of contemporary populations can be shown to reflect migrations in the past.[193] In these areas, our work has only just begun.

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[188] For the Americas, see the estimate for arrivals in Klein (1999) 209 (9.5 million from 1662–1867), and further findings concerning mortality in the Middle Passage (139–40) or the sex ratio of shipped slaves (161–2). No such data are available for the Roman world: see Boese (1973); Harris (1980, 1999) on the Roman slave trade. Occasional references to enslavement totals do not tell us about overall levels of transfers: e.g., Harris (1979) 59 n. 4 (297–293 BCE); Ziolkowski (1986) 74–5 (217–167 BCE). For rough estimates for Republican Italy, see Scheidel (1999c) 111–12, (1999d); and cf. (1997c) 167 for a guess of the volume of the cross-border slave trade under the early empire.

[189] See above, n. 147–52.

[190] Parkin's unwillingness to address this subject is representative of recent research: Parkin (1992) 135–6. There is a brief section in Frier (2000), prudently without any attempt at quantification. Sordi, ed. (1994) is a good example for the very limited utility of non-quantitative work on this issue. For pertinent bibliography, see Horden and Purcell (2000) 616–8.

[191] Wierschowski (1995), based on 680 transfers of 659 individuals recorded in 640 inscriptions; see 279–323 for his data. By comparison, about 13,000 persons are known from Gallic inscriptions (20). He concludes that the upper classes were more mobile (between cities); and in fact, a fifth of his specimens are *equites*, *decuriones* or priests (271). However, and contrary to his assertions, it is far from obvious that the rural population was largely immobile: compare Osborne (1991). This epigraphic approach cannot measure village-to-town mobility, nor the slave trade; in general, it misses migrants too poor to set up tombstones.

[192] Pertinent work in progress includes the 'Progetto Isola Sacra' of the Museo Nazionale Preistorico Etnografico 'Luigi Pigorini' in Rome, a study of the cemetery of Portus.

[193] For Italy, see esp. Cavalli-Sforza, Menozzi and Piazza (1994) 277–80, drawing on Piazza *et al.* (1988); see also Rickards *et al.* (1992), but cf. Rickards *et al.*

Population size and population change

The question of the size of populations, rather than their age and sex composition or rates of growth, has traditionally been given pride of place by ancient historians with demographic interests. In this regard, ancient demography differs quite significantly from the study of more recent populations. This abiding infatuation with local and aggregate totals is best explained with the relative abundance of pertinent references in Greek and Roman literature. But appearances are deceptive: in general, the reporting of numbers in ancient texts can be shown to be very heavily controlled by literary conventions. While some figures may well be based on accurate counts, the large majority of references are no more than symbolic values, at best indicative of a certain order of magnitude and deployed to lend colour or emphasis to the author's exposition.[194] The lack of any systematic analysis of the construction and uses of numbers in classical literature sheds an unflattering light on the persistent unwillingness of ancient historians to come to terms with the insuperable deficiencies of their sources. Henige's recent aptly titled study of 'numbers from nowhere', concerned with estimates of pre-contact American populations but with numerous asides on the classical tradition, is required reading for anyone dealing with the problem of Greek or Roman population tallies.[195] Moreover, it is easy to forget that even if factually correct, bare counts are of limited value unless assessed in context.[196]

(1998) for abiding difficulties of interpretation. For a first attempt to apply this material to the study of Roman slave demography, see Zelener (1999), with Scheidel (1999d). In any event, the genetic evidence qualifies the sweeping rejection by Horden and Purcell (2000) 380 of the view that in the pre-modern Mediterranean, 'most country folk were the grandchildren of country folk in the same region'.

[194] Several studies of prices and other amounts of money in Roman literature clearly prove this point: see Duncan-Jones (1982) 238–56; Scheidel (1996d); Duncan-Jones (1997). Analogous studies of military tallies are still missing. For rhetorical stylisation in other types of counts, see Dreizehnter (1978); Fehling (1990); Rubincam (1991).

[195] Henige (1998a), a devastating critique of modern attempts to estimate American population size and change before and after 1492; for discussions of ancient data, see 17–22, 25–7, 214–42 (again in (1998b)), 245–8, 251–3, 258–61. The ancient historian in a hurry should at least consider his brief description of playful attempts to calculate the total number of the Elves and Orcs of Middle Earth from hints in J. R. R. Tolkien's oeuvre (287–8, drawing on Loback (1987, 1990)), which bear an uncanny resemblance to modern calculations of Athenian manpower in the classical period based on recorded troop strengths and similar serious academic exercises.

[196] Cf. Sallares (1991) 48: 'The size of a population considered in isolation at a

The basic method of estimating the size of local or regional populations in the ancient world has remained substantively unchanged since the days of Beloch: figures reported in ancient sources are checked against likely levels of carrying capacity, and accepted, modified or dismissed accordingly.[197] The only alternative to the wholesale acceptance or rejection of the ancient tradition, this approach permits us to weed out implausible or impossible claims but suffers from the problem of defining the concept of 'carrying capacity'. Since patterns of land use and crop yields can only be guessed at, calculations of carrying capacity are usually fraught with considerable margins of error.[198] In the case of urban populations, we can only wonder to what extent the aggregate 'carrying capacity' of their catchment area (including sources of imports) may have exceeded the yield of their own city territories.[199] Total population size, urban population size, yields per unit of land and levels of urbanisation are four interrelated variables none of which is properly documented for any Roman community.[200]

Nevertheless, population size matters, and is of more than antiquarian interest. In fact, it can be critical for our perception of the history of some of the most important communities of the classical world. Thus, our views on the true extent of political participation and military mobilisation in classical Athens or its dependence on food imports from overseas depend on our estimates of its gross population. To an even higher degree, the fortunes of classical Sparta were determined by changes in manpower.[201] Among Roman historians, the debate has largely revolved around the problem of the size of the citizenry in the late Republic. Other issues, such as the

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point in time is a parameter which is of no significance and has little meaning because data on size alone say nothing about the balance of mortality and fertility which regulates changes in size. In ecology, population size only becomes of real interest when it is combined with area to yield data for population density, in which form it becomes useful for biological models of carrying capacity'.

[197] On this method and its application by Beloch's forerunners—J. Letronne in 1822 and H. Clinton in 1824, on Greece, and Dureau de la Malle (1840), on Rome—, see Gallo (1990) 138–9. For recent attempts to expand Beloch's findings using the same approach, see Corvisier (1980), (1991) 229–92.

[198] Cf. Sanders (1984) for an extreme example.

[199] E.g., Hopkins (1983c); Engels (1990) 27–8, 79–84 (see below, n. 239); Pleket (1994).

[200] See below, at n. 236–42.

[201] Athens: e.g., Hansen (1985, 1988, 1994), on the political system and warfare; Garnsey (1998) 183–95, on food supply, with a survey of recent controversies in my addendum (ibid., 195–200). For Sparta, see Hodkinson (1989).

number of slaves in Roman Italy or the empire as whole, have proven even more difficult to tackle. With regard to Italy, we may distinguish between three distinct but ultimately interconnected issues: the size of the city of Rome; the total number of Roman citizens; and the total number of slaves of Italy. Any estimate of the aggregate tally for Italy is a logical corollary of our assumptions concerning each of these segments.

Modern estimates of the metropolitan population are constrained by the amount of the built-up area and depend in the first instance on the quality and consistency of literary evidence for the grain dole and the number of recipients of other forms of public largesse. By and large, recorded figures converge in support of a relatively high maximum of the order of 800,000 to 1,000,000 inhabitants.[202] Thus, a million is conventionally taken to represent a credible peak value, sufficient to turn imperial Rome into the largest European city prior to London around 1800. This nascent consensus has recently been challenged by Storey on the grounds that the implied levels of population density are far too high by the standards of comparative evidence.[203] In defence of the 'high' estimate, Lo Cascio uses statistics from Italian cities in the eighteenth to twentieth centuries to show that an average density of 40–50,000 residents/km^2 was not unusual.[204] Hence, and especially since early imperial Rome may have extended beyond the confines of later fortifications, a grand total of 800,000 to 1,000,000 in an area of 15 km^2 or more implies an acceptable mean of perhaps 50–60,000.[205] Lo Cascio also confirms that ancient records all point to a similar target figure and are therefore unlikely

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[202] For the main arguments in favour of this range, see Hopkins (1978) 96–8; Morley (1996) 33–9. The more detailed study by Lo Cascio (1997) 23–36 reckons with 440,000–600,000 metropolitan citizens under Augustus; if we add aliens and slaves, this new estimate approaches the same range, especially if we allow for further growth after Augustus. Earlier discussions and estimates are legion: see, e.g., Salmon (1974) 11–22 for a lower minimum of 500,000, exceeded to an unknown extent. Constraints of living space result in an upper limit of about one million: Packer (1967) 87. The general critique by Maier (1953/54) is still useful. Estimates of the size of the population of Constantinople in late antiquity also rely on information on the volume of the food supply: e.g., Mango (1985); Durliat (1990); Sirks (1991).

[203] Storey (1997), pointing out that a population of one million within the perimeter of the third-century CE Aurelian Walls yields an average density of *c.* 72,000 inhabitants per square kilometre, compared to *c.* 17,000 in Pompeii and *c.* 32,000 in Ostia, and likewise compared to *c.* 29,000 in Rome in 1931 or *c.* 43,000 in Manila in 1980.

[204] Lo Cascio (forthcoming c), on Rome, Naples and Trieste.

[205] Compare the likely mean for Roman Alexandria of *c.* 60,000/km^2 (*c.* 500,000 on 8.25 km^2); see Delia (1989).

to be collectively inaccurate and the product of government propaganda, as Storey insinuates. Two guiding principles emerge: it is important to select *appropriate* comparative data, and imprudent to question convergent claims in ancient sources from a variety of backgrounds and periods. From a demographic perspective, the size of Rome matters as a principal determinant of the volume of migration and hence of population change in rural Italy. Urban attrition is the other major variable in this equation: only a large *and* unhealthy capital could have acted as a powerful 'population sink'. For this reason, interpretations of urban and rural population size and change are inextricably tied up with the contentious issue of urban excess mortality, to which I have referred above.[206]

Assessments of the total number of Roman citizens depend entirely on a small number of recorded census results, many of which—at least from the third century BCE onwards—may be based on documentary evidence of actual population counts but were transmitted by literary texts or more rarely inscriptions. Earlier numbers, reaching back to the beginning of the Republican period, are now widely treated with due scepticism but on occasion still attract defenders.[207] Rough but more realistic estimates of the number of Roman citizens or their allies can only be derived by way of extrapolation from probable carrying capacity.[208] In any event, it is the late Republican and early imperial tallies that matter most.[209] Here, the debate has long oscillated between two competing and mutually incompatible positions. The attested census totals jump from 900,000 or 910,000 in 69 BCE to 4,063,000 in 28 BCE and comparable values in the following decades.[210] Under the Republic, only men of fighting age

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[206] See above, n. 106.

[207] The prudent caution of Brunt (1987) 26–33, who advocates scepticism regarding any reported figures prior to the First Punic War, is lacking from the account of Ward (1990), which is in essence a reprise of Frank (1930), who thought that earlier figures, while obviously too high to refer to adult men only, covered the entire citizen population: Ward not only displays astonishing trust in the annalistic record but even echoes Frank's racist prejudices. Hinard (1991a) 109–10 also uses the early Republican data.

[208] The most recent set of estimates is conveniently available in Cornell (1993) 204–8 (*c.* 35,000 Romans by the late sixth century BCE, following Ampolo (1980)); 351 (*c.* 350,000 Romans by 338 BCE, following Afzelius (1942) 153); 380 (*c.* 900,000 Romans by 264 BCE).

[209] The fullest account so far has been provided by Brunt (1987) 44–120. Lo Cascio (forthcoming a) promises a comprehensive rebuttal and re-assessment.

[210] Livius, *Periochae* 98 (900,000) and Phlegon FGrHist 257 F 12 (910,000) for 69

(seventeen years and over) were enumerated in the census. However, given that under Augustus, most Roman citizens must still have resided in Italy and indeed mostly in its peninsular half, the reported totals of four to five million imply a gross population of Roman Italy—including women, children, aliens and slaves—that would be difficult to reconcile with comparative evidence from the early modern period. Not only is the resultant grand total very high to begin with, we also need to allow for a certain amount of underreporting (inevitable even in modern censuses) and further population growth during the early Principate. Beloch, thanks to his unique expertise in later Italian population history, was acutely aware of this problem, which he sought to solve by assuming that the Augustan census figures included the entire citizen population instead of adult men only. Though challenged by Kornemann at the end of the nineteenth century and again by Frank in the 1920s, Beloch's reading was later endorsed by Brunt's massive study of Roman manpower of 1971 and has until recently been almost universally accepted as the most plausible interpretation.[211]

Depending on our reading of the early imperial census figures, the Roman citizenry at the turn of the millennium *either* numbered four or five million (or maybe a million more, allowing for some underreporting) *or* was approximately three times as large. There is no obvious middle route between these dramatically divergent estimates.[212]

BCE; *Res Gestae Divi Augusti* 8 (4,063,000 for 28 BCE, 4,233,000 for 8 BCE and 4,937,000 in 14 CE); Tacitus, *Annales* 11.25 (5,984,072 for 47 CE). Unless the imperial censuses were manipulated by the authorities, they must undercount the actual population, as all censuses do, especially in premodern settings, but we cannot tell by what margin.

[211] Beloch (1886) 370–8 and again (1903). *Contra*: Kornemann (1897); Frank (1924); Jones (1948) 7; and cf. also Wiseman (1969) 72–5. In favour of Beloch: Brunt (1987), with (e.g.) Bernardi (1977) 97–8; Hopkins (1978) 33, 68; Parkin (1992) 5; Morley (1996) 46–50; Scheidel (1996a) 93; Salmon (1997) 120–1; Frier (2000). Rich (1983) also operates with Brunt's figures. It deserves attention that a reverse shift in counting practices (shortly after 338 BCE) was postulated by Frank (1930) 313. Cf. also Salmon (1974) 24.

[212] Judging by its results, the most appealing alternative is offered by the assumption that the Augustan figures include adult men and women but not children: thus E. Cavaignac, cited by Salmon (1974) 29 n. 17 In this case, the age-limit is drawn at 12 or 14 years, in keeping with later provincial evidence. However, as the Egyptian census returns show, even in the provinces everyone may have been registered even if only adults were taxed. Russell (1958) 72 advances the characteristically arbitrary view that only men and women from the age of 17 were covered by the Augustan census. According to Pieri (1968) 182, the Republican figures excluded *proletarii*; however, this would have no bearing on the present issue.

Neither scenario appears particularly compelling. The Augustan change in reporting practice is not directly attested; the population total produced by Beloch's interpretation seems low relative to the large number of cities in Roman Italy—supposedly in excess of 400, although outside Italy some of them might have counted as large villages, as for instance in Egypt—and implies exceptionally high levels of urbanisation even excluding the capital itself.[213] As Lo Cascio shows in this volume, the 'low' estimate also implies staggering levels of military mobilisation.[214] Moreover, it requires us to assume that the free Italian population outside Rome shrank slightly during the first century BCE, owing largely to emigration to the capital and the provinces. On the other hand, the 'high' total is very high indeed: according to my own calculations, it boosts the population of peninsular Italy outside Rome to a level equivalent to that close to the end of the nineteenth century.[215] Even so, however implausible, this result need not be entirely impossible.[216]

Since 1994, Lo Cascio has repeatedly argued in favour of the 'high' estimate suggested by a traditionalist reading of the reported census figures. He has advances his thesis in two complementary

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213 Lo Cascio (2000b).

214 See Lo Cascio in this volume. Hopkins (1978) 31–7, who uses the word 'staggering' (35) for his estimate that on average, about 60 per cent of all seventeen-year olds were drafted for 7 years of service (or 44 per cent for 10 years, etc.) during the last two centuries of the Republic. For the Augustan period and beyond, see the corresponding calculations in Scheidel (1996a) 93–7 (15–22 per cent of all Romans aged 20 joined the armed forces under Augustus).

215 Scheidel (1996a) 168.

216 Average yields of 15 *modii* (129.3 litres) of grain for human consumption and 5 *modii* for seed per *iugerum* (0.25 ha) produce 2.59 billion litres of grain per year on 100,000 km^2 of arable under conditions of universal biennial fallow, enough to feed 8.5 million people at 300–310 litres per year. The extent of fallow is of crucial importance: two years of cropping and one year of fallow could support 11.3 million consumers; three years of cropping and one year of fallow feed 12.8 million. An increase of the share of arable from 40 to 45 per cent raises the totals further, to 9.6 million, 12.7 million, and 14.4 million, respectively. Thus, in theory, and assuming that the capital was fully supplied from overseas, the total population of Italy could have been as high as 15 or 16 million. In that case, however, northern Italy had to produce a net surplus that could be transferred to the peninsula: there is no evidence of such mechanisms. Neither average seed/yield ratios nor fallow practices are known. Under these circumstances, calculations from carrying capacity perforce remain unhelpfully rough, although they make it hard to believe that the population of Italy outside Rome could have been closer to 15 than to 10 million.

ways, by attempting to show that the 'high' total is possible and more frequently by focussing on those logical implications of the 'low' estimate—such as extreme levels of urbanisation and military mobilisation—that render Beloch's scenario (even) less plausible than the 'high' alternative.[217] This is not the place to judge the merits of either position, all the more so as Lo Cascio's full presentation of his argument is still outstanding.[218] Instead, it is worth drawing attention to telling similarities in method on both sides of the debate: both scenarios are anchored in a tiny set of documented figures which are subjected to different interpretations and checked against comparative evidence selected in accordance with the proposed result: while some statistics from the more recent past suggest that the 'high' estimate is anachronistic in terms of overall population size, others highlight possibly anachronistic features of the competing model, above all with regard to urbanisation and mobility. In this respect, the controversy over the number of Roman citizens closely resembles the discussion of the size of the population of the city of Rome: in both cases, scarce source references are interpreted through the lens of conflicting samples of comparative data.

The problem of determining the gross population of Roman Italy is compounded by our ignorance of the number of slaves. Modern estimates are scarce and invariably disappointing, down from over twenty million in the early nineteenth century to less incredible but equally unsubstantiated two or three million at present.[219] It is disconcertingly easy to show that these latter guesses, which have gradually usurped the status of fact and now form a commonly accepted conventional range,[220] are based on nothing.[221] This is particularly troubling since estimates of the slave population are not merely relevant for our understanding of the Roman economy but also crucial to the debate over the size of the citizenry. Thus, an actual slave total

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[217] Lo Cascio (1984a,b), (1996) 289–91, (2000b).

[218] Lo Cascio (forthcoming a). *Contra* Lo Cascio, see Morley (1996) 48–50; Scheidel (1996a) 167–8. Lo Cascio, in this volume, seeks to refute some of these objections; but see Scheidel (forthcoming d). Informal unpublished reactions indicate less-than-universal rejection of this revisionist model.

[219] Blair (1833) 15–16 (see above, n. 18); Beloch (1886) 436 (2 million) (cf. 415–8: 1.5 to 2 million); Brunt (1987) 124 (3 million).

[220] E.g., Hopkins (1978) 102 n. 8; Finley (1998) 148; Patterson (1982) 354; Bradley (1994) 12; Scheidel (1997c) 158 n. 17; Klein (1999) 3; Scheidel (1999c) 109–12.

[221] See Scheidel (1999d, forthcoming e).

for Roman Italy of significantly less than two million would undermine the 'low' estimate: the net decline of the rural citizen population in the late Republic envisaged by this model needs to be offset by a massive influx of foreign slaves. As a matter of fact, Brunt conjures up a slave population of three million precisely in order to arrive at a credible total of 7.5 million for Italy as a whole.[222] However, as long as estimates of servile population numbers are merely a function of interpretations of the number of citizens and not corroborated by independent means, they are of little practical value.[223]

This weakness shows in Hopkins's speculative model of the population of Roman Italy under Augustus which is based on the 'low' estimate for the citizenry and Beloch's guesstimate of two million slaves: one in three inhabitants of Italy was a slave; fully half of the urban population outside the capital was made up of slaves, but only one-third in Rome itself.[224] Without all these slaves, it would be difficult to assign a sufficient number of residents to the towns of Italian without raising the total number of citizens beyond the limits of the 'low' estimate. Then again, we cannot rule out the possibility that slaves were indeed concentrated in the cities and that their presence in the countryside has been overestimated, perhaps in analogy to modern plantation slave societies.[225] In a radical departure from traditional modes of investigation, Zelener has argued for a considerably smaller slave population in Roman Italy by pointing out that the genetic makeup of the current Italian population is inconsistent with the presence of millions of slaves in antiquity.[226] However, even in the absence of original statistics, a grand total in excess of one million slaves can be shown to be plausible.[227] At the end of the day, this is a problem of method—of how to devise the least fallacious way of making the most of inadequate evidence.

Historical demographers familiar with more recent societies are bound to be stunned by this lack of uncertainty about the most basic measures: there could never by any disagreement over whether the

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222 Brunt (1987) 125; cf. Scheidel (1999d).

223 For a similar approach, cf. Gallo (1984) 70, calculating the number of Athenian slaves from the discrepancy between the size of the citizen population and the food supply.

224 Hopkins (1978) 68–9.

225 Thus Jongman (forthcoming).

226 Zelener (1999); *contra*: Scheidel (1999d).

227 Scheidel (forthcoming e).

population of England in 1600 was closer to four or to ten million (a level not reached until the early nineteenth century). They might reasonably conclude that under these circumstances, certain issues are simply not worth pursuing. However, our vision of Roman society is crucially determined by its demographic background, although Roman historians have shown little awareness of just how much hinges on our interpretation of the early imperial census figures. The 'high' estimate brings Roman urbanism in line with the experience of other pre-modern societies, makes Rome look less like early modern London, undermines the classical tradition concerning the paramount importance of chattel slavery and concomitant claims about upheaval and desolation in the Italian countryside (which have already been qualified by recent progress in survey archaeology), and forces us to reconsider crucial episodes of Republican history from the agitation of the Gracchi to the Civil Wars of the 40s and 30s BCE. The 'low' estimate, by contrast, calls for a much more dynamic model of social and economic transformation along the lines envisioned by Hopkins, resulting in massive population transfers and surprisingly high levels of slave-driven urbanisation. In a nutshell, whereas the 'high' estimate renders Roman Italy exceptional in terms of overall population density, the 'low' estimate makes it look exceptional in almost every other respect. Either way, the heartland of the Roman empire must have been exceptional. This puts severe constraints on the use of comparative evidence: in this context, simplistic reasoning from analogy is bound to be seriously misleading. As a result, we may doubt whether a universal consensus can ever be reached.[228]

Much of this debate has been replicated for Roman Egypt. All we have are two incompatible population totals arrived at under unknown circumstances: although it is certainly possible that the Roman administration added up the tallies of local tax registers and census counts to produce a grand total, we cannot be sure if any of the figures reported in literary sources are based on official statistics of this kind.[229] Once again, reasoning from carrying capacity

[228] In Frier's view ((1999) 101), 'this controversy will not be resolved for many years, if not decades, to come'.

[229] Diodorus 1.31.6–9 (3 million); Josephus, *Jewish War* 2.385 (7.5 million without Alexandria); critiqued by Rathbone (1990) 103–7 and defended by Lo Cascio (1999a). Recent colloquia initiated by C. Nicolet have focused on the lost archives of the classical world and their statistical materials.

is fraught with uncertainties and comparative data have been marshalled in support of significantly divergent estimates from four or five to more than eight million inhabitants.[230] The resultant dilemma bears an uncanny resemblance to the problems concerning the population of Roman Italy: while an estimate of four or five million seems too low given the number and size of the Egyptian cities of that period and also compared to levels of population density in the early nineteenth century,[231] a total in excess of eight million implies a degree of populousness equivalent to that close to the end of the nineteenth century but reduces the proportion of the non-agricultural population to more plausible levels—in much the same way as the 'high' estimate of Roman citizens does for peninsular Italy.[232] Even though the evidence for the number of Roman citizens is structurally unrelated to references to Egyptian population numbers, I am not aware of the existence of any recent commentator favouring the 'low' estimate in one case and the 'high' total in the other: the majority view, represented by Beloch and Frier, gives preference to 'low' figures for both Italy and Egypt, whereas Lo Cascio argues for the alternative positions. Internal consistency is obviously an issue here: extremely high levels of urbanisation in Egypt would conflict with the view that they did not exist even in Italy while the assumption that the population density of Egypt in the Roman period approached that of the late nineteenth century might undermine the contention that nothing comparable could have happened in Roman peninsular Italy. However, with regard to Roman Egypt, unlike in the case of Italy, many scholars have been prepared to abandon the reported figures—which lack the clout of the Roman citizen census—and devise their own intermediate estimates.[233] My own argument for a

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[230] Carrying capacity: van Minnen (1997) 39–40, 42–3. Comparative data: Rathbone (1990) 107; Bagnall and Frier (1994) 53–4; Lo Cascio (1999a) 161–3. If the Egyptian censuses of the nineteenth century are anything to go by, there is no good reason to place much trust in ancient population tallies: e.g., McCarthy (1976); Panzac (1987). For the first comprehensive assessment of the predictive value of the nineteenth-century data, see Scheidel (forthcoming a) ch. 3, and cf. briefly (1999e).

[231] These 'low' estimates forces us to reckon with very high levels of urbanisation: Rathbone (1990) 123 (20 to 25 per cent); Bagnall (1993) 312 (30 per cent); Bagnall and Frier (1994) 55–6 (37 per cent, or 29 per cent excluding Alexandria). For criticism of the highest of these rates, see Bowman (1995) 311; Hopkins (1995/96) 64–5 n. 4; van Minnen (1997) 40–1.

[232] Lo Cascio (1999a) 169. For Italy, see above.

[233] E.g., Beloch (1886) 258; Issawi (1981) 377, 379; Rathbone (1990) 108; Bagnall

range from five to seven million as a likely population maximum for early imperial Egypt results from a critical assessment of the often flawed and incomplete population counts of the nineteenth century and the differences between the disease environments of these two periods.[234] In the final analysis, it is our approach to modern sources that is crucial: though usually chosen and cited in a haphazard manner in support of preconceived views, the application of demographic evidence from the more recent past calls for the same amount of critical circumspection as the interpretation of ancient references.

Egyptian papyri furnish us with unique documentary evidence of local population numbers. Rathbone's study of the size of various cities and villages in Roman Egypt has become an instant classic; further attempts at synthesis have been undertaken by Bagnall, van Minnen and Alston.[235] In forthcoming work, Tacoma will introduce a new way of extrapolating the likely size of Egyptian district capitals from the known size of their territories. There can be no doubt that the urban population of Roman Egypt was very substantial by any historical standard and fairly evenly distributed across the country. This observation underlines the need for broader models of urbanisation and urbanism, elaborating on the recent studies by Alston and van Minnen. Proper appreciation of the demographic peculiarities of Roman Egypt will be essential for any realistic reconstruction of its social and economic development.

Outside Egypt, the absence of documentary statistics is the norm. As a consequence, the number of inhabitants even of the best-known Roman city, Pompeii, must needs remain controversial.[236] While Jongman reminds us that 'estimating Pompeian population remains a hazardous game' (both for the city and its territory), Wallace-Hadrill

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and Frier (1994) 56, 103. 'Not claiming to provide more than a convenient model', van Minnen (1997) 40 accepts the high figure because he regards it as supported by independent calculations.

[234] Scheidel (forthcoming a) ch. 3.

[235] Rathbone (1990); Bagnall (1993) 52–3; van Minnen (1997); Alston in this volume.

[236] Modern estimates range from *c.*7,000 (Russell) to *c.* 20,000 (Nissen): see the discussion in Jongman (1988) 110–12; for the urban population, Jongman favours the estimate of Eschebach (1970) 66–7, of 8–12,000 (112), as does Wallace-Hadrill (1991) 203 (10,000 'purely for the sake of hypothesis'; see next footnote). Storey (1997) 973 calculates a total of *c.* 11,000. Ostia has been the target of an even wider range of estimates, from 20,000 to 100,000 (discussed by Meiggs (1973) 532–5); recent calculations converge on 22–27,000 (Storey 974 and n. 5).

goes even further in rejecting the 'pursuit of purely chimerical absolute figures' in favour of exploring 'what consequences the assumptions we make on the macro level about the total population of the city have on the micro level for the population of the household'.[237] Occasional glimpses can do little to remedy this situation. In the Republican period, we are sometimes told the strength of the original contingent of settlers setting up new colonies but remain ignorant of the urban/rural split and the subsequent evolution of these communities. Other than that, isolated reported figures from different parts of the empire are often difficult to interpret.[238] Calculations from carrying capacity or analogy with later periods risk circularity.[239] Schematic extrapolations from seating capacity of Roman theatres are highly dubious; assumed ratios of seats to residents have varied from 1:1 to 1:4.[240] Other possible indicators, such as the volume of

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[237] Jongman (1988) 111; Wallace-Hadrill (1991) 203 = (1994) 98, and see his methodological misgivings about any such estimates in (1991) 199–203 and (1994) 95–8.

[238] For a list of pertinent references, see Duncan-Jones (1982) 260–1 n. 4. Interpretations differ and misreadings have been known to occur: see, e.g., Cumont (1934) 189, wondering whether an epigraphic reference to 117,000 'citizens' of Apamea excludes the majority of the population below an unknown census threshold; Warren and Bagnall (1988), on a misread text; Simon (1993); and compare Salmon (1974) 34 with Harris (1999) 65 n. 18 on Galen 5.49 ed. Kühn.

[239] For instance, see McCown (1947) and again Broshi (1979) for low population estimates for Roman Palestine relative to Josephus's inflated figures, but cf. Safrai (1994) 436: 'it is impossible to determine how many people lived in the Land of Israel during the Mishnah and Talmud periods'. Probabilistic extrapolations from carrying capacity are turned on their head by Engels (1990) 79–84, on Roman Corinth, who attributes an enormous built-up area of 5 to 7 km^2 to the city and its port, then argues that the implied urban population of somewhere close to 100,000 could not possibly have been fed by Corinth's territory; but see Saller (1991).

[240] For criticism of such estimates, see Gallo (1981) 272–4; Duncan-Jones (1982) 262; Henige (1998a) 252, 403 n. 59. Fusshöller (1991) dismisses previous guesses of the urban population of Thamugadi of *c.* 16,000 (based on a 1:4 ratio of seats to residents) and puts the number of city-dwellers at *c.* 4,000 (later rising to maybe 6,000) based on the size of the built-up area, the number of residential units, and the probable carrying capacity of the territory (*c.* 50 km^2 of arable, enough to support 4–6,000 people according to local comparative data suggesting a mean density of 80 people/km^2). This raises the questions of why a community of 4–6,000 should have built a theatre with 3,500–4,000 seats, and whether each inhabitant of the territory can be thought to have lived in the city (cf. Garnsey (1998) ch. 7 for Italy). Likely levels of population density at Pompeii (120–180 people/ha) point to a total of 1,400–2,100 for the city of Thamugadi (11.5 ha); Fusshöller's own estimate improbably renders this African town more densely packed than Ostia. This indicates that calculations based on the number of blocks or units in the city can be seriously misleading (compare Woolf (1998) 137–8 for fairly low densities in cities

the water supply or the scale of *summae honoriae*, are even less reliable.[241] In the case of a few cities in Italy and Africa, Duncan-Jones has been able to calculate the number of residents from epigraphic records of the scope or the number of recipients of municipal benefactions: however, even these sources sometimes leave room for doubt.[242]

The numerical strength of particular groups within Roman society is an even more intractable issue. The total number of Roman slaves is completely unknown. Harris's global guess of from one-sixth to one-fifth of the gross population of the empire, first advanced in 1980 and recently re-iterated, is best understood as symbolic quantification of his view that slavery was widespread throughout the provinces.[243] The census records from Roman Egypt are the only type of evidence that permits us to calculate the proportion of slaves in the sample populations. According to the data studied by Bagnall and Frier, most of them from Middle Egypt, 10.9 per cent of registered individuals are slaves, as many as 13.5 per cent in the cities but only 8.5 per cent in the countryside.[244] However, more recently discovered documentation from a less affluent community in Upper Egypt which attests an urban share of seven per cent suggests that the earlier database need not be representative of Egypt as a whole.[245] Reckoning with an ever lower proportion of slaves in southern villages, the average share of slaves in the total population of Egypt outside Alexandria may not have exceeded six or seven per cent.[246] Yet with conditions in Lower Egypt virtually unknown, this new estimate remains rather shaky. Moreover, puzzling hints at substantial slave numbers in some other provinces from Africa to Asia and Syria compel restraint in the application of this 'low' estimate for Egypt to other parts of the Roman world.[247] Although a significantly more

in Roman Gaul). In sum, Fusshöller's study illustrates the variety of obstacles to modern estimates of urban population size.

[241] Duncan-Jones (1982) 261.

[242] Duncan-Jones (1982) 259–77. In some cases, the scope of the distributions—only the city or the entire territory—remains unclear.

[243] Harris (1980) 118, (1999) 65.

[244] Bagnall and Frier (1994) 48, 70 n. 69. Biezunska-Malowist (1977) 156–8 estimates slaves as about 7 to 11 per cent of the non-Alexandrian population.

[245] Bagnall, Frier and Rutherford (1997) 98.

[246] Scheidel (forthcoming e, f).

[247] E.g., Apuleius, *Apologia* 93, with Gutsfeld (1992) 252, but cf. Scheidel (1996d) 237 n. 34 (Africa); *IGRR* 4.914 (Asia); Johannes Chrysostomus *PG* 7.608, with Whittaker (1987) 96–7 (Syria).

limited presence of slaves than envisaged by Harris is perfectly possible (and in my own view plausible),[248] the margins of uncertainty are simply too wide to settle this question.

The size of religious communities has also received a fair amount of attention. Claims about the number of Jews in the Roman world invariably lack solid foundations.[249] A few years ago, Wasserstein put paid to the common view that it had run to seven million, or at least one-tenth of the gross population of the empire at its peak.[250] If anything, this figure seems far too high.[251] Stark and Hopkins's simple model of the numerical development of early Christianity, anchored in a few superficially plausible assumptions, demonstrates the superiority of parametric modelling in the face of missing evidence:[252] their interstitial guesstimates have important implications for the evaluation of ancient sources (above all of rhetorical claims exaggerating the spread of the new faith)[253] and our understanding of the social structure and intellectual capabilities of the early cult groups.

In view of all these uncertainties, it is hardly surprising that the question of long-term trends in population size poses sheer insuperable difficulties. Where our sources cannot be trusted to provide us with population numbers that are both precise and accurate for any particular moment in time, serial analysis remains illusory. The only time-series of population counts consists of literary and epigraphic references purportedly drawing on the results of Roman citizen cen-

[248] See Scheidel (1997c) 158 for a guess of a 10 per cent share of slaves in the imperial population, rejected by Harris (1999) 65; cf. Scheidel (forthcoming e).

[249] See Noethlichs (1996) 10 with 151–3 n. 64–68 for a recent survey of estimates and references.

[250] Wasserstein (1996) 313–4: 'We cannot hope to establish any reliable estimates of absolute numbers or of relative proportions. We cannot even be sure of the orders of magnitude involved. (. . .) We must resolutely resist the temptation to formulate any precise statistical statement of the absolute number of the Jewish inhabitants of the Roman Empire in the first century, and, equally, any precise computus of their proportion in the total population of the Empire.' What is ultimately the only source underlying modern 'estimates', a claim in the thirteenth-century (!) chronography of Bar Hebraeus that 6,944,000 Jews were counted under Claudius, was clearly caused by a misunderstanding of the reported total of Roman citizens in Claudius's census: see the discussion in Wasserstein 309–12 (one textual variant for the latter puts it at 6,944,000).

[251] Harnack (1924) 13 reckoned with 4–4.5 million Jews, again without proper evidence. Cf. now also Hopkins (1998) 213–4 (*c.* 3 million?). On Palestine, see above, n. 239.

[252] Stark (1996) 6–7, followed by Hopkins (1998).

[253] See Hopkins (1998) 188–91, on Plinius, *Epist.* 10.96 and other texts.

suses. However, as I have noted above, while the early tradition has largely been cast aside, the late Republican and early imperial figures are subject to fundamental disagreements.[254] We can only guess at the growth rates of the Republican city of Rome.[255] The scale of its subsequent decline during the late imperial period and beyond has also generated much inconclusive debate.[256] Dynamic processes will forever remain largely impervious to more than speculative schematic quantification.

Estimates of the overall number of inhabitants of the Roman empire in its prime are heavily indebted to Beloch's valiant efforts to estimate the likely population of different provinces from a melange of source references and probabilistic computations of carrying capacity. This method yields a tally of fifty to sixty million in the early first century CE, a result later depreciated by Beloch himself but only cautiously modified in more recent studies.[257] Frier allows for aggregate growth by one-third within the 150 years following the death of Augustus. While this translates to an average annual growth rate of 0.15 per cent for the empire as a whole, net growth is thought to have been largely confined to western Europe and North Africa.[258]

[254] See above, esp. n. 211, 217.

[255] E.g., Starr (1980) 15–26 (*c.* 30,000 around 350 BCE, *c.* 60,000 around 300 BCE, *c.* 90,000 by 270s BCE), cf. Cornell (1995) 385; Morley (1996) 39 (200,000 in 200 BCE 'simply as a working figure'); Brunt (1987) 384 (*c.* 375,000 by *c.* 130 BCE?).

[256] For a recent survey, see Lo Cascio (1997) 58–76, largely based on references to the number of recipients of food handouts; he argues for an urban population of well over half a million in the fourth century CE and a drop from the fifth century onwards. See also Barnish (1987); Lo Cascio (1998); and cf. Hodges and Whitehouse (1983) 51. Attempts to estimate the population of late Roman Italy as a whole are fairly hopeless: see very briefly Hannestad (1962) 68–9.

[257] Beloch (1886) 502 (50 to 60 million by 14 CE), 507 (54 million as the sum of his estimates for individual provinces). Always allowing for a higher total in the following centuries, especially in the western and northern provinces, Beloch initially resisted the temptation of further quantification ((1886) 502) but in (1899a) 620 offered a higher total of 100 +/− 20 million for the late second century CE. (It seems to me that the lower end of this range need not be wide of the mark; see below.) Among others, see, e.g., Russell (1958) 71–86, on various provinces, and Salmon (1974) 23–39 for a discussion of later revisions and estimates. Cf. also the useful conspectus of estimates in Jones (1996) 262. Drawing on McEvedy and Jones (1978), Frier (2000) presents hypothetical sets of provincial population figures for 14 and 164 CE, totalling approximately 45 and 60 million, respectively. As Hopkins (1995/96) 67 n. 22 points out, Lo Cascio's argument in favour of 'high' totals in Italy [and now also in Egypt] seems to imply a grand total of 'well above 60 million'. For Hopkins's attempt (1995/96) 47 to discredit Beloch's later total of 100 million, see below, at n. 311.

[258] Frier (2000), see the previous footnote. Growth net of annexations is put at

Indeed, recent studies have repeatedly suggested that Beloch's estimates for certain provinces in 14 CE may not be representative of the situation in the second century, most notably in the case of Egypt and Gaul.[259] As a result, Frier's proposed maximum of about sixty million may have to be revised upwards, although Beloch's guess of somewhere close to 100 million for the later Principate still seems excessive. While the grand total for the entire empire is arguably less crucial to our interpretation of Roman history than the size of the Republican and early imperial citizen population, more detailed knowledge of the former would be invaluable in assessing the degree of urbanisation in various parts of the empire, the numerical relationship between the general population and the armed forces,[260] changes in the relative population density of the western and eastern halves of the Mediterranean between the Roman and later periods,[261] and the demographic specifics of the transition to the Middle Ages.

It is the last of these themes that has traditionally received most attention: the 'decline' of the (western or entire) Roman empire in late antiquity, not only in political and military but also in economic and demographic terms. In the absence of serial statistics, demographic contraction can only be illustrated (though never proven) by means of potential proxy data: smaller cities, abandoned land, smaller families. Two separate but quite often conflated issues are at stake: is it true that the population of the Roman empire, or of its western half, shrank significantly between the first (second, third) and the

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12.3 per cent in the East and 42.2 per cent in the West. While this imbalance seems plausible, evidence indicative of further growth in the eastern provinces during late antiquity complicates the picture (see below, n. 268).

[259] Egypt: see Scheidel (2000) ch. 3, arguing for a peak range from 5 to 7 million. Frier (2000) favours a peak of 5 million. Gaul: the estimates of Drinkwater (1983) 168–70 and King (1990) 105–8 imply a maximum of 14 to 16 million, whereas Woolf (1998) 138 more plausibly settles for a peak of around 12 million. Frier gives 9 million.

[260] E.g., MacMullen (1980, 1984); Goldsmith (1984); and esp. Hopkins (1995/96) 46–8. But see below, at n. 311.

[261] In 1949 and 1966, Braudel (1995) 394–8 reckoned with a similar total of 60 to 70 million in roughly the same area (minus some marginal regions) during the late sixteenth century, some 54 to 63 per cent of them in Portugal, Spain, France and Italy and the remainder in the Islamic Balkans, Anatolia, Greater Syria, Egypt and North Africa. Beloch's split for 14 CE assigns 37 per cent to the Latin West minus Africa; Frier has 47 and 52 per cent in the Latin West minus Africa in 14 and 164 CE, respectively. Precise figures for either period would enable us to trace the westward shift of power and demographic resources after 1000 CE (cf. Thomson (1998) 190). For Roman and later distributions of population within Italy, see Jongman (1988) 72–6.

fourth (fifth, sixth) centuries; and if it were true, would it matter?

The principal obstacles to answering the first question are immediately apparent. Smaller cities need not be less populous, only more compact; even if they are, the rural population need not be affected in the same way. The global, average development of cultivation and population density is impossible to measure: shifts in settlement patterns may reflect nucleation or diffusion rather than net contraction or growth. Most importantly, average reproductive performance—above all, discrepancies between the fertility rate and the gross reproduction rate—remains totally unknown: there are no relevant primary statistics, and they cannot be reconstructed from the existing record.[262]

Archaeological proxy data are inevitably drawn from individual spots, mostly cities and their territories or at best small regions. Studies of this kind unfailingly reveal local and regional diversity. Thus, the fortification of exposed cities in the western and Balkan provinces was necessarily accompanied by a reduction of their perimeters, while urban sprawl continued in more peaceful regions.[263] It is true that the collapse of the western Roman empire and the contraction of the early Byzantine empire eventually affected the well-being of urban communities throughout the Mediterranean world.[264] However, the strength of the 'ancient city' was probably determined by its political and economic functions rather than gross population size.[265] It is even more difficult to identify, let alone quantify, demographic change in the countryside.[266] Intensification and abatement

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[262] See above, n. 130.

[263] See Liebeschuetz (1992) for the evolution of the late antique city and the factors affecting it, such as modes of tax collection, elite spending, trade flows, and subdivision of territories; see esp. 8–11 on physical contraction and its possible meanings. He concludes that 'the development of the Late Roman city in East and West is a story of infinite variety' (35). See also Fischer (1993) for a recent survey, revealing regional variation but no universal crisis in the third century CE.

[264] E.g., Poulter (1992); Reece (1992). In North Africa, urban decline coincided with the Vandal takeover: Lepelley (1992) 68. On the end of the ancient city in Egypt, see Alston in this volume. On Italy, see Christie (1996).

[265] See Alston in this volume. Compare also Marsden and West (1992), on the apparent contraction of London and other cities in Britain after the mid-second century CE which—if real—would be hard to explain except with reference to economic change. The Weberian/Finleyan concept of the 'ancient city' is still debated: e.g., Whittaker (1993b); Parkins, ed. (1997).

[266] Field surveys suggest interpretations but are by nature inconclusive. I doubt that the ambitious five-volume 'Populus Project' (Bintliff and Sbonias, eds. (2000) and sequels), a self-described 'major study of long-term demographic changes in Mediterranean Europe using the techniques of landscape archaeology', can change

are not in and of themselves reliable markers of prosperity or failure, of population growth or decrease.[267] Nevertheless, the overall picture is one of vigour and sometimes even expansion in the eastern provinces of the fourth through sixth centuries: while urban centres continued to attract immigration, the occupation of marginal land has been taken as a sign of continuing population pressure.[268] Much the same is true of Latin North Africa and also Egypt.[269] Even in the West, prior to the fifth century, evidence of rural depression is largely confined to exposed frontier regions.[270] In the East, significant decay appears to have occurred only in the later sixth and the seventh centuries, a development associated with a pandemic of bubonic plague and political transition.[271]

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this. The pertinent literature is considerable: see, e.g., Barker and Lloyd, eds. (1991). Alcock (1993) 33–92, esp. 53–5, stresses the criterion of visibility, which complicates extrapolations from site counts to population size: different settlement patterns produce different landscapes.

[267] Horden and Purcell (2000) 265–6. Christie (1996) gathers evidence suggestive of urban and rural decline in Italy in late antiquity and beyond, but adds important qualifications: 'It is a problem, however, to determine how this situation resulted from, on the one hand, over-exploitation of the land, excessive deforestation, or simple bad management or, on the other hand, from social disorders such as increased taxation, depopulation, or insecurity. Combinations of the factors, plus climatic deterioration, must also be considered: it is unlikely that monocausal explanations will be valid even for one region since each was a patchwork of owners, all with their own level of money, resilience, and skill' (279).

[268] For a summary, see Liebeschuetz (1992) 25–34, and also briefly Cameron (1993) 177–82. On the cities, see Patlagean (1977) 179–81. On the countryside, see Tate (1992), esp. 273–342, documenting uninterrupted growth from the late third to the mid-sixth centuries in clusters of villages in northern Syria east and south-east of Antioch: by 550, between three and eight times as many rooms existed as in 350, varying with region. A sudden drop followed, possibly caused by the plague; Tate thinks that by the sixth century, the region had come to hold as many people as it could support, exposing them to positive Malthusian checks. A similar expansion is attested for the Hauran in southern Syria: Dentzer, ed. (1985), esp. Villeneuve (1985), on growth from the first century BCE to the fifth (?) century CE. See also the comments by Foss (1995).

[269] On Africa, see Lepelley (1979–81), and more recently and concisely Lepelley (1992), with further references. On fourth-century Egypt, see Bagnall (1993).

[270] The ambitious survey of Lewit (1991), a study of 201 excavated or closely surveyed rural sites in Italy, Britain, Gaul and Spain, finds no evidence for widespread abandonment in the third and fourth centuries. In the fourth century, occupation levels equalled or exceeded early imperial levels in southern Gaul, Spain and Britain. Lower density in northern Gaul and Belgica may reflect a shift away from dangerous areas. The apparent parallel downturn in Italy is more difficult to explain. Whittaker (1976) tackles the tradition for *agri deserti*, which are a common phenomenon of Mediterranean history in general. See also Moreland (1994).

[271] Liebeschuetz (1992) 34–5.

More often than not, the demographic development of the later Roman empire is considered interesting not so much for its own sake but as a factor in the imperial collapse.[272] In this scenario, depopulation and wholesale decline go hand in hand: manpower shortage is part and parcel of a moralising view of history with roots in Greek and Roman literature and followers up to the present.[273] I have already commented on this ideological bias in the section on fertility control. Suffice it to add that a full-scale study of this tradition from antiquity to the present has long been overdue.

Regardless of the reality of late Roman depopulation, the implied causal link to systems collapse is far from self-evident. Gibbon's famous dictum that under Louis XIV, a single former Roman province was capable of supporting an army larger than that of the entire Roman empire shows the way. If military strength were a mechanical function of gross population size, history books would be much slimmer, and we would all speak Chinese. Had the Roman state, in the third or fifth centuries CE, achieved levels of short-term military mobilisation comparable to those attested for the mid- and late Republic, we would still speak Latin, instead of Chinese. Long-term shifts from the seasonal mobilisation of peasant-citizen farmers to the employment of professional soldiers and on to the enlistment of geographically marginal groups are unrelated to the size of the gross population under Roman political and fiscal control. Early China provides an instructive parallel: according to Elvin, following the replacement of peasant soldiers by professionals under the Han empire, 'the heavy cost, relative to the total output of food and

[272] Demandt (1984) 352–65 provides a much-needed survey of previous scholarship, with ample references. A well-known example of this approach is Boak (1955a), criticised by Finley (1958). Russell (1958) 140 regards the entire period from 200 to 600 CE as a time of population decline. Salmon (1974) 114–79 is superficially more cautious, hedging his bets in his concluding remarks 178–9; in Whittaker's words ((1976) 200 n. 113), 'Salmon appears to favour the general notion of population decline while admitting there is no evidence for it which will stand up to examination'. For a recent attempt to revive this scenario, though this time with respect to the crisis of the third century CE rather than late antiquity, see Wierschowski (1994), with blatant disregard (357–8) for Finley's incisive critique of the Boak thesis. See above, n. 156, 161.

[273] See Gallo (1980) for a study of this motif in Greek thought; he finds that an abundance of population (polyandria) was associated with prosperity and strength, scarcity of people (oliganthropia) with decline and weakness; military decline triggered notions of demographic contraction. For later associations of depopulation with fertility control and moral degeneracy, see above, n. 167. Cf. also Frier in this volume, on early modern concerns about oliganthropia.

goods, of maintaining the administrative superstructure, and of providing the soldiers and supplies necessary for imperial security', began to undermine the system. By contrast, the Sung empire of the eleventh century, with a population of well over 100 million, was able to support a standing army of 1.25 million, whereas an army a quarter that size 'would have been enough to bring the empire to financial ruin' under the Han, who ruled over perhaps half as many subjects.[274] Hence, in addition to levels of popular mobilisation, productivity and average per capita surpluses are further critical determinants of military power. Nevertheless, even the superior Sung empire was eventually brought down by far less numerous peripheral 'barbarians'. All this suggests two important points. First, in the absence of significant technological (or epidemiological) imbalances (Diamond's 'guns, germs and steel'),[275] military superiority is primarily a function of social organisation and economic performance. And second, the fate of the Sung empire suggests that even higher surpluses, let alone a larger gross population per se, would not necessarily have ensured the survival of the Roman empire.[276] The collapse of complex societies requires more sophisticated explanations.[277]

Comparative evidence makes it hard to identify forces that may have caused significant long-term demographic contraction in the Roman empire. Analyses of fluctuations in both European and Chinese history have come to place emphasis on factors as varied as political stability and climatic change.[278] Epidemics matter only if they occur on the scale of the Black Death and/or if they regularly recur over several generations.[279] Thus, although major epidemics in the

[274] Elvin (1973) 19–20, 84.

[275] Diamond (1998).

[276] In this connection, the comparatively small number of the successful Germanic invaders merits attention: according to Burns (1978), by 500 CE no more than 35–40,000 Goths had settled in Italy; compare the estimates reported by Jones (1996) 267 (maybe not more than 20,000 warriors). 80,000 Vandals supposedly reached Africa, unless this figure is exaggerated, as it may well be the case (cf. Russell (1958) 77; Jones (1996) 268); at any rate there is no good reason to trust it (Henige (1998a) 17–22). Are we to believe that a Roman empire of, say, 70 or 80 million instead of 50 or 60 million would have been more adept at dealing with a few tens of thousands of Germanic warriors?

[277] Tainter (1988). Compare briefly Doyle (1986) 98–103 and Mann (1986) 283–95 for two sociological analyses of the fall of the western Roman empire that do not even raise demographic issues.

[278] See Galloway (1986, 1994) for the impact of climatic change: see below, at n. 320–6. Chu and Lee (1994) associate population loss in pre-modern China with social unrest and political turmoil; but see below, n. 284.

[279] See Livi-Bacci (1992) 44–50 on the Black Death and later plague waves in

late second and the mid-third centuries may have resulted in noticeable population losses,[280] the empire ought to have recovered by the early fourth century. Bubonic plague, potentially a powerful agent of demographic contraction, emerged too late to account for the disintegration of the western empire. The impact of climatic change would necessarily vary across an area as diverse as the Roman empire, interfering with agricultural production in some regions whilst improving conditions in others. Further studies of this subject are badly needed.[281] Without total war or genocide, the extent of interruption of life in the countryside must not be exaggerated.[282] Famines are usually of minor demographic relevance.[283] Last not least, as I have tried to show above, one of the most popular culprits, widespread or universal below-replacement fertility due to excessive family limitation, is a mere fantasy. (And even if it were not, it could have had the beneficial effect of increasing per capita surpluses.)

All in all, there is no compelling reason to assume that the gross population of the Roman empire, or even of its western half, was significantly smaller in the fourth century than at the beginning of the imperial period or even in the second century, nor is it obvious that any such population loss, had it indeed occurred, would have significantly contributed to the crisis of the fifth century. Roman historians might profit from closer consideration of the specific circumstances of demographic contraction in other, better documented societies: unfortunately, this is a topic that has been curiously neglected by historical demographers.[284] As Horden and Purcell put it in their

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Europe. Lesser epidemics have fewer repercussions: cf., e.g., Wrigley and Schofield (1989) 209 on an 8 per cent contraction of the English population between the 1650s and the 1680s, with a full recovery by the 1710s. However, lower net growth may have prevented similarly swift recoveries in antiquity: at 0.15 per cent annual growth, it would take 70 years to offset a 10 per cent drop. In general, recovery rates vary depending on social factors, as indicated by Thomson (1998) 103–7.

280 See above, at n. 121–5.

281 For an exemplary case study, see below, n. 324.

282 See Hanson (1998), and Desy (1993), an optimistic appraisal of the economy of Apulia during and after the Second Punic War. See also Brunt (1987) 269–77.

283 See Watkins and Menken (1985), esp. 665. In an extreme case, the famine in China in 1959/61 during the 'Great Leap Forward' which caused 14 million extra deaths, the population recovered within three years: Kane (1989) 236 table 12.4.

284 Brent Shaw and I hope to address this issue in a collaborative project. In any event, reliable data are hard to come by, as the example of China shows: to judge from the chart in Chu and Lee (1994) 354 fig. 1, Chao and Hsieh (1988)—available only in Chinese—reckon with sudden and violent swings in population size from the third century BCE to the fourteenth century CE, sometimes by up to

new Braudelian history of the ancient and medieval Mediterranean, 'worries about rural depopulation have been common enough to generate a suspicious multiplication of supposed crises over the centuries: if they are pivotal events, Mediterranean history has been singularly rich in pivots.'[285]

Instead of chasing the phantom of demographic contraction, it may be more promising to explore the possibility of *over*population and population pressure in the Roman empire. This alternative, ignored by most Roman historians but central to the concerns of historical demographers of other pre-transitional populations, was first mooted by Lo Cascio and is now more systematically set out by Frier in this volume.[286] In this scenario, slow but steady growth during the early empire could have stepped up the pressure on scarce resources; high fertility might even have contributed to high mortality. Thus, the temporary mortality crises of the second and third centuries might simply have helped redress a growing imbalance between production and consumption. The growth of cities may also have served to reduce the pressure by exposing surplus population to higher urban mortality levels.[287]

This is a theoretical model. We cannot be sure whether overpopulation had actually become a serious problem. Demographic expansion, especially in the West, would have resulted in fruitful intensification and the exploitation of previously underutilized resources, above all land, before running up against diminishing returns on additional input (as in western Europe in the early fourteenth century). Proxy data, such as material evidence of urban growth, are

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two-thirds of the total. However, it is acknowledged that reliable statistics have only been available since the fourteenth century, and the fact that no comparably dramatic fluctuations are attested from that time onwards should give us pause. Previous assessments of Chinese population change were more sceptical of these supposed swings; see Chu and Lee (1994) 368–9 for a brief discussion.

[285] Horden and Purcell (2000) 266.

[286] See below, Chapter 4; also Frier (1999) 103–5, and at the end of Frier (2000). It deserves notice that Frier's discussion is based on 'low' estimates of the imperial population (see above, n. 257). The 'high' estimates of Lo Cascio (see above, n. 217, 229) necessarily imply relative overpopulation, above all in Italy: see Lo Cascio (1994b) 119–25, (1996) 293–7.

[287] The city of Rome is often cited as the best example: Sallares (1991) 89; Hopkins (1995/96) 60; Morley (1996) 39–46 (although he classifies the pull of unhealthy cities as a 'burden' rather than a benefit); Scheidel (forthcoming d); for objections, see Lo Cascio (2000a, forthcoming c). The cities of Roman Egypt may have served a similar function: see Scheidel (forthcoming a) ch. 2.4. For the general question of urban excess mortality, see above, n. 106.

consistent with this scenario but cannot not strictly speaking confirm it.[288] References to competition over marginal land in early imperial Italy have been taken as indicative of population pressure.[289] It remains unclear whether long-term price trends in Roman Egypt reflect demographic change or unrelated factors, such as monetisation or inflation.[290] In the East, expansion into marginal land seems to have gained momentum in late antiquity, which might be taken to suggest that population pressure had thus far been relatively weak.[291]

To judge from their telling concerns about 'depopulation', Roman historians unfamiliar with comparative evidence tend to regard population growth and high population density as intrinsically desirable and beneficial. However, even though population pressure may trigger technological innovation and changes in land use that raise productivity,[292] it is far from clear that the Roman world ever experienced progress along these lines.[293] At the very least, and regardless of

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[288] On the rise of urbanism in Roman Gaul, see Woolf (1998) 106–41. The growth of urban settlements does not automatically suggest overall population growth: while growing cities need to be supported by their hinterland, rationalisation may allow a smaller number of producers to feed a larger non-agricultural population.

[289] Evans (1980) 34–5, on conflict over *subseciva* near the borders of communities and over appropriation of sacred land due to *densitas possessorum*; see also Dyson (1992) 115. In the *ager Veientanus* under the Principate, settlement was spreading into marginal land: Potter (1979) 133–4. However, Evans's view that Domitian's 'wine edict' might have responded to a shortage of subsistence crops is less plausible: see Patterson (1987) 116–8 for its moralistic character.

[290] Frier (1999) 104 and (2000), drawing on data in Duncan-Jones (1990) 143–55, considers the possibility of population growth reflected by an increase in wheat prices from the first to the second century CE. However, as Frier also notes in this volume, Rathbone (1997) finds no significant distinction between first and second century price levels, reckoning instead with broad price stability for wheat between the mid-first and the late second centuries. There was a more gradual increase in the cost of wine. Cf. below, n. 303, for the connection between prices and crisis. Rathbone 216 stresses that further studies of the price history of other commodities, such as land, rents and houses, are required to reconstruct a more complete picture of overall trends. Drexhage (1991) has gathered pertinent data.

[291] See above, n. 268. If the increase in rooms in the villages studied by Tate (1992) reflects demographic growth, a threefold increase (a minimum average) over 200 years implies an annual growth rate of 0.55 per cent, barring migration. Alternatively, it may also reflect a shift to more substantial lodgings. For lateral expansion as a response to population pressure, see Chisholm (1968) 127–31.

[292] The classic works are Boserup (1965, 1981); see also more briefly Grigg (1980) 29–39.

[293] In Frier's words ((1999) 105), 'it would still take a brave soul to argue, on the basis of present evidence for the state of the early imperial economy, that such increased rationalization could possibly have sufficed to offset any major surge in population levels'. The case-studies in Grigg (1980) 49–234 show not only that population pressure could result in either progress or crisis, but more significantly that

whether population pressure really was an issue, Roman historians would be well advised to lay to rest the (often implicit) assumption that more is usually better and less is worse: a pre-modern society such as ancient Rome was more likely to suffer from overpopulation than from a shortage of hungry mouths.

Demography and economy

For medieval and modern historians, demographic change is a major factor in economic development. Students of the Roman economy, by contrast, have hardly ever considered the potential relevance of the demographic background. Pleket's denouncement of this unjustifiable negligence has thus far failed to have any visible impact on the field.[294] Frier voices similar concerns in this volume. In this regard, the gulf between the conceptual worlds of historians of the 'ancient economy' and economic historians of more recent periods is particularly wide.

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responses to demographic growth that were successful in the long run are only known from post-medieval Europe. Population growth without corresponding economic development is not beneficial: see, e.g., Thomson (1998) 108–9, on that kind of demographic growth in south Italy and Sicily in the later seventeenth and eighteenth centuries: 'the region's economic structure was such as to make dire poverty and glaring inequality a permanent feature. The agriculture of the region, which provided the only basis on which growth could take place, was deprived of any source of investment by an exploitative landholding structure and urbanization pattern. Though its livelihood came from the land, the region's elite preferred to reside in the city. Naples was the largest city of Europe, containing between 440,000 and 540,000 inhabitants, but its economy depended almost entirely on rural transfers.' This sounds like a perfectly plausible scenario for much of the Roman empire.

[294] Pleket (1990) 47: '[es] fehlt in der Geschichte der römischen Kaiserzeit überraschenderweise der demographische Ansatz beinahe ganz. In der Theorie des Primitivismus werden in Übereinstimmung mit dem statischen, wachstumsfernen Gesamtcharakter, der dieser Epoche zugeschrieben wird, demographisches Wachstum und Bevölkerungsrückgang sowohl als Phänomene wie auch in ihrer Rolle als potentiell einflußreiche Faktoren übergangen'. Cf. also Sallares (1991) 51, on Greece. The neglect of demography is not even mentioned in Morris (1999), an assessment of reactions to Finley's work on the ancient economy (XXIII–XXXI); and except for a brief aside (13 n. 15), population issues are also missing from Harris (1993), a survey of aspects of the Roman imperial economy deserving of further study. For first reminders, see Duncan-Jones (1980) and very briefly Hopkins (1983) XV–XVI. The next general synthesis of the ancient economy will accommodate demographic factors: Scheidel (forthcoming c).

The nature of the evidence is partly to blame: there are no solidly documented series of data on prices or climate that could be correlated with equally well-attested demographic events.[295] The interaction of demographic change and economic growth is the pivotal issue. Did population pressure spur economic development, or vice versa?[296] Should archaeological evidence of agricultural intensification be interpreted as indirect evidence of population growth or of rising per capita product?[297] Did genuine population loss, caused by severe epidemics, improve access to land and boost wages, thereby raising the living standards of the survivors? In principle, all these questions are crucial to our understanding of the Roman economy. In practice, we are often reduced to conjecture and analogy beholden to comparative data. Pleket, in his pioneering if unfocused overview of demographic and economic change in the Roman empire, can do no more than alert us to possible implications and problems of interpretation.[298]

Even so, kites have occasionally been flown. Safrai, in his recent study of the economy of Roman Palestine, presents a tentative model of economic growth under population pressure. Increasing settlement densities in hitherto marginal areas, the establishment of satellite settlements, the introduction of new plant species and expansion in the

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[295] Compare, e.g., Rotberg and Rabb, eds. (1986).

[296] Cf. above, n. 292–3. The Malthusian model that population levels are to some extent determined by technology, and the Boserupian model that technology itself is changed by population pressure, are not only contradictory but also complementary in so far as they share the basic premise of diminishing returns to labour for a fixed technological level: see Lee (1986) for an attempt at synthesis. For the view that economic development facilitates demographic growth, see Morris (1994) 363–4, on the 'new model' for classical Greek agriculture, associating signs of agricultural expansion and intensification in sixth to fourth century BCE Greece with production for the market and 'greater overall prosperity'; this development is thought to have 'fueled demographic expansion'. Cf. 365 for independent indicators of greater prosperity.

[297] Claims about economic growth need to account for the difference between aggregate and intensive (per capita) growth: an increase in total output does not necessarily translate to an increase in per capita production and consumption. Cf., e.g., Hitchner (1993), on the expansion of North African olive production.

[298] Pleket (1990) 55–70, esp. 55–9 on demography, with heavy reliance on Brenner's arguments (Brenner (1982)) though without reference to the subsequent debate: see Aston and Philpin, eds. (1985). I hasten to add that in terms of comparativist awareness, Pleket's contribution stands head and shoulders above the other chapters in Vittinghoff, ed. (1990).

cultivation of existing ones (above all flax, supporting linen exports), specialisation in cash crops and the gradual increase in the number of urban centres up to the sixth century are all taken as telling markers of this process.[299] While unable to decide whether population pressure prompted economic development or vice versa (if this is a meaningful distinction at all), he considers the former more likely.[300]

The reverse scenario has also begun to attract attention. Sharp investigates the possible impact of the 'Antonine plague' on the agricultural economy of the village of Theadelphia in the Fayum.[301] To judge by rudimentary local statistics, it seems that the amount of cultivated land dropped by twenty to twenty-five per cent between the years immediately preceding the epidemic and a generation after its end. Moreover, viticulture expanded at the expense of arable, suggesting that after the epidemic, less grain was needed to support a smaller number of residents who enjoyed higher living standards than before.[302] Similar developments in western Europe after the Black Death proffer an inviting parallel. However, the view that the smallpox pandemic of the late second century may have brought long-term economic benefits similar to those of fourteenth-century bubonic plague is hard to reconcile with evidence of higher price levels long after the epidemic had subsided. Rathbone has attributed the structural doubling or more of prices in the late second century to 'unprecedented economic dislocation' which was caused by the epidemic and boosted both food prices and wages. The governmental response of debasing the coinage then helped stabilise prices and wages at this new, higher level.[303] The short-term 'dislocation' evoked

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[299] Safrai (1994) 446–57, referring back to more extensive discussion in the previous chapters, and 438–9 figs. 102–3 for the rise in the number of sites. See also Safrai (1998), tracing growth or decline in different parts of the country during the fourth century; decline in the fifth; and recovery in the sixth. For comparable evidence of rural expansion in Syria, see above, n. 268.

[300] Safrai (1994) 457–8. Cf. also (1998) 131.

[301] Sharp (1999) 185–9.

[302] Sharp (1999) 188: before, 1,600–1,870 ha of arable and *c.* 140 ha of vineyards and orchards; after (216 CE), 990 ha of arable and 410 ha of vineyards and orchards. Prior to the epidemic, the supposed total population size of *c.* 2,500 (Sharp 164) tallies well with the amount of cultivated land (assuming that ninety per cent of all teenage and adult men cultivated *c.* 2.5 ha/person: cf. Cuno (1999) 316 for this ratio). This means that before the epidemic, the land was exploited at full capacity; subsequent population loss would have relieved the pressure.

[303] Rathbone (1997) 215–6, with a full analysis of price data for wheat (190–8),

by Rathbone is in fact quite well documented: as mentioned above, Duncan-Jones has discovered evidence of temporary disruptions of economic activity during the plague years in various parts of the empire.[304] This is a characteristic side-effect of major mortality crises which initially affect production and services even more heavily than they reduce actual population size. In western Europe, wheat prices rose in the immediate aftermath of the Black Death—fifteen years after the outbreak, an Italian observer claimed that 'most things cost two times or more what they cost before the epidemic'[305]—, then remained high for about a generation (and locally up to twice as long) before commencing a long decline. At the same time, wages rose, as did standards of living among farmers and labourers.[306] In Roman Egypt, by contrast, the crisis-induced price increase became a permanent fixture. No medium-term benefits are discernible in third-century data; unlike in late medieval Europe, food prices and wages did not begin to move in opposite directions.[307] Highly conjectural calculations suggest that in terms of the wage/price ratio, Egyptians may even have been somewhat worse off from about 190 to 250 than for most of the second century.[308] A quick recovery of the population—in late medieval Europe prevented by further disease waves[309]—could have contributed to this development. Official monetary and fiscal responses to the crisis which 'helped to fossilise these price rises into the new higher but stable bands of prices which lasted to around 274' may also have been responsible.[310] That the

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wine (198–206) and donkeys (207–10), all of which reflect a steep rise between *c.* 160 and *c.* 190 CE.

[304] See above, n. 122.

[305] Matteo Villani in 1363, quoted by Herlihy (1997) 46–7.

[306] E.g., Herlihy (1997) 47–51. See also Dyer (1998) 307, and the striking chart in Lee (1987) 447 fig. 2.

[307] The scarcity of price data for approximately half a century after the end of the epidemic hampers our analysis: all we can say with confidence is that from *c.* 240 CE onwards, both daily wages and wages were higher than in the second century. For these data, see Drexhage (1991) 402–39.

[308] However, the margins of error are considerable: see Drexhage (1991) 440–54, esp. 444, for a discussion of change in the cost of living from the first to the third centuries CE. It also seems that Egyptians may have enjoyed greater prosperity in the first century (444), conceivably reduced by subsequent population growth.

[309] E.g., Dubois (1997) 212–5.

[310] Rathbone (1997) 216, with reference to coin debasement, the reduction of small denominations and the abandonment of set prices for state purchases and valuations of wheat. Cf. Carrié (1997) for long-term price trends in Roman Egypt from the first to the sixth centuries.

epidemics of the 250s and 260s failed to trigger a corresponding price jump may indicate that they were less severe than the 'Antonine plague' or that we have misunderstood the cause of the previous increase.

Hopkins has taken a different track in essaying to gauge likely gross population size from economic performance. According to his schematic calculations, if the Roman empire was home to sixty million people, minimum GDP was at least nine billion sesterces and actual GDP up to one-and-a-half times as large. Under these circumstances, an annual budget of 700 to 900 million sesterces, of which 450 to 500 million was spent on the army, translates to an average tax rate of five to seven per cent of actual GDP. However, if the gross population had been significantly larger, 100 instead of sixty million, the implied mean tax rate of three to four per cent of GDP would have enabled the government to raise taxes in times of need and support a much stronger army. 'An enemy attack would have induced a minor tax hike, and a massive increase in central government resources. This did not happen. It is therefore easy to conclude that the empire did not have at its disposal a population of 100 million people.'[311] Hopkins's point is well illustrated by the case of France under the *ancient régime*: in 1700, tax revenue equalled 5.1 per cent of GDP, comprised of 3.1 per cent of agricultural production and 10.8 per cent of industry and trade. By 1789, the overall share had risen to 8.7 per cent, with 6.3 per cent for agriculture and 13.9 per cent for the other sectors.[312] However, this example also makes it seem plausible that state revenues from farming, as a share of total output, may have been similarly low in the Roman period. It is at least conceivable that the late Roman state, unlike the last French kings, lacked the power to raise this share beyond a three or four per cent level; moreover, the numerical evolution of the late Roman army remains controversial.[313] As a result, estimates of gross population size from probable levels of gross production and state revenue are of little value in demarcating the limits of the plausible.

[311] Hopkins (1995/96) 46–7 (quote: 47).
[312] Goldstone (1991) 205.
[313] MacMullen (1980) holds that overall troop strength changed little from the late Principate to the late empire, but reckons with a drop in quality. It seems that the garrison of Britain was reduced after the Severan period: Jones (1996) 214.

My focus on the relationship between economic development and population numbers could easily create a misleading impression. As a matter of fact, demographic functions from mortality to fertility and family structure are equally relevant in this context. Just as high mortality was crucial in determining the social realities of Roman family life,[314] its influence on investment and economic activity in general also merits consideration.[315] Beyond the economic sphere, the effect of mortality on the status of widows and orphans or the impact of high fertility on the status of women likewise call for attention.[316] Demographic conditions are among the main variables that help us measure something as important and at the same time elusive as standards of living or wellbeing.[317]

Demography as ecology

Sallares's ambitious work on the ecology of the ancient Greek world has the potential of lifting the study of ancient ecology, and thus of ancient history in general, onto a new level of interdisciplinary complexity and sophistication. Thus far, however, this model has failed to encourage similar efforts. Roman historians have been particularly reluctant to engage in the debate. It remains to be seen whether Horden and Purcell's new study of ancient and medieval Mediterranean history will help remedy this deplorable situation. More than half of Sallares's book is devoted to various demographic aspects from fertility control to disease. An analogous study of the ecology of Roman Italy is a desideratum of the first order.[318] Only studies that endeavour to situate and examine the demographic regime within a wider ecological framework can hope to overcome the sterile decontextualisation of formal demography. Disease and nutrition are only a

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314 Saller (1994), esp. his Roman kinship simulations (48–65).

315 For a first attempt, see Saller (1999).

316 See Krause (1994–95), on widows and orphans in the Roman world; Saller (1994) 189–203, on Roman guardianship; Demand (1994) 102–20, on female acculturation to early childbearing in ancient Greece; and cf. in general above, n. 168.

317 See Frier (forthcoming).

318 Sallares (1991), esp. 42–293 on demography; cf. Scheidel (1996c) for a preliminary assessment of published reactions to his work. Compared to Sallares's study, subsequent publications in this field, such as Hughes (1994) and Shipley and Salmon, eds. (1996), signify indifference and regression. Horden and Purcell (2000) promise a more extended discussion of demographic features in a forthcoming sequel.

few of the subjects that need to be fully integrated into Roman population studies. In the section on mortality, I have drawn attention to ongoing research on causes of death in the Roman world. Sallares's new study of the impact of malaria on the demography of central Italy in the Roman period and beyond is an important step in the right direction.[319]

Climatic change as a determinant of demographic conditions also requires further attention. Galloway has tried to show that long-term fluctuations in the gross population size of pre-industrial societies in middle-latitude areas vary with mean temperature; warmer periods coincide with population growth.[320] This observations enables us to consider demographic change without reference to proximate causes, such as social, cultural or religious factors. It is generally agreed that the period of the late Republic and much of the empire was characterised by relatively high mean temperatures, followed by a noticeable downturn in late antiquity.[321] Agricultural production may have expanded and contracted accordingly.[322] This lends support to the view that the early Roman empire may have experienced population growth that had an adverse effect on the quality of life and proved unsustainable in the long run: in general, demographic expansion stimulated by higher temperatures tends to continue beyond the raised carrying capacity threshold and hence result in a higher equilibrium with lower living standards.[323] Any subsequent reversal of climatic amelioration and attendant environmental degradation would inevitably undermine this precarious balance.

A careful recent discussion of the role of climatic change in the loss of Roman Britain offers a model for analogous studies of other parts of the empire.[324] According to Jones's survey, Britain benefited greatly from the clement climate of the early imperial period until,

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[319] Sallares (forthcoming) on malaria, and work in progress on leprosy; see also Scheidel (forthcoming d), on disease and demography in the city of Rome.

[320] Galloway (1986).

[321] E.g., Lamb (1981); Greene (1986) 83. Conditions in the High and Late Middle Ages, with a temperature optimum from *c.* 1200 to 1400 and subsequent cooling, provide a useful parallel. For general surveys of climatic history, see Lamb (1977, 1995); Röthlisberger (1986).

[322] See Greene (1986) 81–6 on the relevance of climate for the study of the Roman economy.

[323] Lee (1987) 457.

[324] Jones (1996) 186–243 discusses the link between environmental degradation and the end of Roman Britain, mostly with emphasis on climate (187–236) and some comments on epidemic disease (236–9). He does not refer to Galloway's work which would have supported his argument.

following a drop in temperatures, conditions began to deteriorate from the mid-fourth century onwards. Since the preceding optimum had facilitated expansion into marginal land that was unprofitable under less favourable circumstances, Britain's enlarged population was particularly vulnerable to climatic change. In this respect, the resultant crisis in late antiquity may well have been 'roughly analogous to the transition from the medieval warm epoch into the harsher climate of the later Middle Ages'.[325] Nevertheless, Jones urges caution, stressing that the lack of evidence for massive reforestation in marginal areas speaks against depopulation on a massive scale. What is more, Britain should be regarded as a special case: because of its 'latitude and altitude, its extensive marginal agriculture, and the interaction of weather, yield, and harvest failure, Britain's population and crops were more harvest-sensitive than those of her provincial neighbors'. All we can say with confidence is that while favourable climatic conditions were conducive to economic and demographic growth, their reversal contributed to crisis.[326] His circumspect conclusions are a far cry from sweeping claims about wholesale ecological deterioration and its supposed demographic consequences. It is very well possible that if Britain suffered only to a limited extent, more southerly latitudes would have been even less seriously affected by lower temperatures. Other parts of the empire, such as the semi-arid regions in the East, may even have benefited from cooler weather and increased precipitation. In general, lower temperatures help reduce the prevalence of infectious diseases, above all malaria, and thus morbidity and mortality. For all these reasons, the effects of climate change on population size and demographic structure must have been highly localised.

This shows once again that Roman demography cannot be studied in isolation. Local data samples first and foremost reflect local conditions, which are determined by the physical environment and the specifics of cultural practice. The 'lure of aggregates' can be profoundly misleading: although we will ultimately want to identify general trends and characteristics, we must be wary of broad generalisations from an uneasy amalgam of local data and global models.

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325 Ibid. 225 n. 108. Compare Jordan (1996) 15–19, on deteriorating weather conditions in the 1310s, resulting in major famines, followed by a long-term downturn beginning in the mid-fourteenth century. These events were preceded by a secular warming trend beginning in the twelfth century: see Alexandre (1987).

326 Jones (1996) 226–7, 242–3 (quote).

Re-embedding Roman demography

Demography has rightly been called an 'interdiscipline'.[327] Population studies straddle a variety of academic fields from history, biology, medicine and climatology to archaeology and biological, social and cultural anthropology, bridging the institutional and conceptual divides between the humanities, the social sciences and the life sciences. In view of the serious shortcomings of any particular type of available evidence, research in Roman demography simply cannot afford to neglect any of all these different branches of knowledge and enquiry: in fact, a multidisciplinary approach is necessary to make it at all feasible. Ancient population studies are seriously undertheorised: by merely latching on to the procedural practice of the formal demography of the more recent past (itself under scrutiny from anthropologists, feminists and cultural historians), we run the risk of neglecting the ideological background of the ancient tradition, modern research, and continuing ties between the two.[328] The anthropologist's plea for what has been called 'whole demographies' should fall on fertile ground in a field traditionally so firmly rooted in the study of ideology, mentality and culture as ancient history.[329]

The 'problems' intimated in the title of this chapter have dominated wide stretches of my survey. 'Progress' in Roman demography is best defined as the process of overcoming the dissociation of Roman population studies from the historical demography and ecology of other periods. This process, though increasingly accelerated by the application of basic demographic models and theories and cross-disciplinary perspectives, is still in its early stages. It is our approaches, not the quality of the evidence, that determines the success of our investigations and the validity of our conclusions. At the very least, even if heightened methodological awareness will often fail to make inadequate sources yield significant results, it will help prevent the abuse of such data as do exist and curb fanciful speculations that are oblivious to the most basic findings of other disciplines: recent pronouncements on Roman fertility control and depopulation

[327] Stycos, ed. (1989).

[328] Anachronistic appraisals of Roman fertility control are a case in point: see above, 'Nuptiality, fertility and fertility control'.

[329] Greenhalgh (1995) 12–18. See Kertzer and Fricke, eds. (1997) for recent perspectives in anthropological demography.

readily come to mind. Proxy data, comparative evidence and theoretical models may be poor substitutes for reliable vital statistics. At the same time, they encourage a more holistic perspective, transcending facile categorisation and compartmentalisation. This cross-disciplinary embeddedness is the future for Roman demography.

CHAPTER TWO

THE SEASONAL BIRTHING CYCLE OF ROMAN WOMEN*

Brent D. Shaw

It has long been observed that there are distinctive annual seasonal patterns in the principal life cycle patterns in the growth and surcease of human populations. These great regular seasonal fluctuations in births, marriages, and deaths are events that are of great interest to the historical demographer. Through the patient cumulation and analysis of the data provided by Roman funerary commemorations, it has been possible to reconstruct reasonably reliable data sets for the annual cycles of mortality of the city of Rome itself, and indeed for other selected populations of the western Roman empire, for those time periods when the data are available in relative abundance.[1] By means of the inventive processing of this same type of data, it has also proven possible to reconstruct the main annual seasonal cycles of marriage for Roman women.[2] Two of the three major life-cycle stages of some selected Roman populations have therefore finally been made to reveal their annual cyclical patterns to the patient modern researcher. The big problem that remains to be solved is the annual birthing cycles typical of Roman women in these same populations. Might it also be possible to resolve this problem through some ingenious use of the same data that have been used to reveal the annual seasonal cycles of death and marriage?

In attempting to solve the problem of the seasonal reproductive and birthing cycles that typified Roman women, one should restate

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[1] Shaw (1996); Scheidel (1994a) and (1996a) ch. 4 are the principal modern analyses.

[2] Shaw (1997).

a fact that is now well known to historical demographers. This basic observation is that the distribution of the total number of births in a given year for any specific human population group is usually characterized by distinctive seasonal variations. Put in simple terms, if the annual total of births of any given population is subdivided into monthly periods, the births for that population are usually distributed rather unevenly throughout the year. Furthermore, these annual oscillations in reproduction are not random in nature. For any population group, the 'waves' of reproduction are fairly consistent from one year to the next. The myriad repeated acts of reproduction engaged in by the reproducing population combine to form recurrent annual patterns or seasonal cycles that are typical of that population as opposed to any other.[3] These oscillating patterns in the total number of annual births are characteristic of even modern populations where a significant proportion of all human reproduction is subject to various artificial contraceptive practices (fig. 1). As much as regular seasonal fluctuations in human reproduction are true of modern societies, they are even more pronounced in premodern populations. For most human populations in the premodern Mediterranean—generally for times before the end of the nineteenth century—the seasonal oscillations in birthing cycles were generally more exaggerated than they are for the modern populations in this same region today (e.g., compare figs. 2, 3, and 10).

Whatever the causal factors in these seasonal fluctuations in human reproduction might be, they are so deeply rooted in human behavior that their effects can still be observed in hypermodern post-industrial populations. As the problems encountered in a large number of studies on the question that have been published during the last decade have demonstrated, the causes of these profound seasonal shifts in the reproduction of human populations are more difficult to discover

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[3] One of the early comprehensive scientific studies of the problem of seasonal variations in the mortality and reproduction of human populations was offered by Ellsworth Huntington (1938), a work that seems to be assiduously ignored by modern researchers, perhaps because of the ideological connections that Huntington seemed to strike between his data and their possible implications for programs of eugenics. When offering measurements of the degree of the seasonal variations in human populations, including seasonal variability in birthing, demographers translate absolute figures into 'indexes of seasonality' which represent a more general measure of the extent to which the actual numbers for any given month exceed or fall below the mean figures for the year. For a more detailed explanation, see the note appended to this paper.

than might be at first expected. Explanations that have been proffered for these birthing cycles are therefore something to which I shall have occasion to return to later in my analysis. The immediate concern of my inquiry here is to discover what can be known about the seasonal birthing cycle for Roman women. By 'Roman women,' I mean specifically the women of the metropolis of the city of Rome and, more widely, women living in the regional areas of the Italian peninsula during the high period of the Roman Empire, from approximately the first to the fifth centuries CE. Although the cyclical nature of human reproduction must be one of the fundamental data required for a reconstruction of the historical demography of Roman society, the problem of the birthing cycles typical of Roman women has not yet been the object of any thorough investigation.[4] The attempt is surely worth making, since whatever knowledge that we are able to acquire on the seasonal fluctuations in the population will enable us better to understand the cultural and biological aspects of the sexual and reproductive behavior of the human populations that constituted Roman society.

The problems of determining patterns of seasonality in human births are related to broader demographic problems of seasonality in other aspects of human reproduction and life-course. Some of these consistent seasonal patterns are usually seen as biologically or ecologically determined, since they are linked to factors that were beyond the capabilities of pre-industrial and premodern populations to control. Along the spectrum of possible cultural and biological causes, it is above all the seasonal fluctuations in patterns of mortality that would seem to be most directly related to the physical environmental conditions in which Roman people lived.[5] Thus, the differential seasonal rates at which Romans in the city of Rome and in peninsular Italy died—a mortality curve characterized by very high rates of death in the months of late summer and early autumn as opposed to comparatively low rates in the months of winter and

[4] Parkin (1992) has offered a clear and comprehensive overview of the subject as of the date of publication. Many of the problems connected with seasonality are not broached by Parkin—and certainly not the one dealt with in this paper—because it was deemed, with full justification on his part, that there were no obvious data relevant to their resolution. Problems of seasonal demographics of Roman populations have begun to receive attention only very recently, see Scheidel (1994a) and (1996a) ch. 4.

[5] Shaw (1996), where the relevant modern bibliography is discussed in detail.

spring—are assumed to be caused by natural factors, since these Romans, unlike their modern counterparts, were more susceptible to disease bearing vectors (mainly those of temperature and humidity) and to lethal infections that caused much higher levels of mortality to occur in particular seasons of the year.

There are other seasonal patterns in human behavior and demography, however, that seem to be culturally determined. The distinctive seasonal patterns of marriage of women in Roman Italy—with a high frequency of marriage in the winter months of December to February, and low rates in the mid-summer months—would appear to be the result of human cultural choices that produced recurrent seasonal cycles in matrimony. Perhaps it would be more accurate, however, to state that there was an interactive process between environmental constraints, regimes of human labor, and patterns of exchange that were conducive to cumulative 'choices' or preferences of when to marry.[6] Amongst these human cycles, a central question must now occupy us. Where along this spectrum of cause and effect are the seasonal cycles in human reproduction—the seasonal patterning in births—to be located? It might be assumed for the post-contracepting populations of our modern world that reproduction is something that is now more under human control, and that any striking seasonal variations would have been eliminated from our collective reproductive behavior. Given the sum of all choices made by parents who now control their own reproductivity, one might assume that the seasonal variation in births has become distributed evenly throughout the months of the year. Perhaps surprisingly, this does not seem to be the case (fig. 1). In this particular aspect of their reproductive behavior, the populations of the most technologically and economically advanced of postindustrial societies, humans seem to have remained incorrigibly biological and environmentally determined. The seasonal patterns in collective reproductive behavior are so persistently and deeply rooted that they are not easily changed or eradicated by some of the most pervasive and powerful forces that have shaped the modern world.[7]

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[6] Shaw (1997), for the nature of the problem and reference to the salient modern bibliography on the subject. I say 'seem' since marriage patterns are linked to environmental factors that determine cycles in the premodern economic régimes that do not make the resulting collective human 'decisions' to marry as wholly cultural as might seem to be suggested by the term.

[7] In some cases, however, such as that of modern-day Japan, tendencies to 'desea-

Analysis of this basic problem concerning the populations of the Roman empire, more specifically those of the city of Rome and of peninsular Italy, has been neglected or avoided—in part because of a lack of awareness of the existence of the problem as one to be resolved and in part because of the nature of the surviving evidence. Even if an awareness of the problem as a problem existed, almost no researcher would have been moved to try to resolve it. Most modern-day Roman historians would know that we lack the requisite data to answer the question, even if one dared to ask it.[8] Generally speaking, the assumption is true. The exiguous number of birth dates for Romans that could be painfully collated out of the existing narrative literary sources would probably be deemed not to be worth the effort. Indeed, such a collation for one of the best documented of all groups, the Roman imperial family (all persons, including wives, children, and close relatives), can be made. The total number of instances, spread over more than four hundred years, is rather disappointing.[9]

Table 1: Seasonal Distribution of Births in the Roman Imperial Family, *c.* 50 BCE–400 CE (N = 69).

J	F	M	A	M	J	J	A	S	O	N	D
9	7	7	5	2	3	3	10	7	3	7	6

Data source: Kienast (1990)

All that can be surmised from this collation *might* be a greater propensity of births in the November to March period, with a singular odd intrusion in August. The numbers are so small, however, that it would not be prudent to assign any particular significance to them. There are no other obvious literary sources that can produce large numbers of birth records. We know that records of births were kept

sonality' or 'aseasonality' of birthing have been claimed, much as with the aseasonal patterns of hypermodern mortality: see Miura (1987) 26 fig. 1, showing a decided trend to aseasonality in the decades between 1960 and 1980.

[8] Nordberg, 'Dates of Birth', § 7 of Nordberg (1963) 69–72, 70 table 16a, made a collection of date available as of about 1960. On the basis of the 524 dates of birth he collated from the epigraphical evidence then available, he felt that 'one can conclude that the period from December to March was the time when the majority of births took place' (72).

[9] The instances were compiled from Kienast (1990): all cases.

in the Roman Empire, and that a citizen in need of such information might be able to appeal to copies of his birth registration.[10] These birth declarations and registrations had been made mandatory for all full Roman citizens by the emperor Augustus and, much later, the emperor Marcus Aurelius extended the requirement for registration to all citizens, even those of illegitimate birth.[11] Persons who happened to live in regions where there were good archival facilities might be able to appeal to these records as proof of citizenship. But it is too much to believe an optimistic assertion that 'when St. Paul alleged his Roman citizenship before the Roman authorities, he must have produced his birth certificate . . . which he doubtless carried with him wherever he traveled.'[12] The more likely circumstance was that the acquisition of such documentary evidence of citizenship was ordinarily obtained only upon request and in response to a specific need.

For those regions of the empire where there was a tradition and technology of record-keeping, such as Roman Egypt, and where propitious environmental conditions have favored the preservation of official records on papyri, modern researchers might hope to acquire extensive banks of the registration of births. Although such birth declarations must have been filed by the administration of the Roman province of Egypt in very large quantities, the actual numbers of them that survive are few and the difficulties of assessing the broader empire-wide significance of Egyptian evidence are well known.[13] The declarations are divided into two broad categories: those made by Greeks and Egyptians, and those made by Roman citizens. The former, alas, are cast in a format that did not require of the declarant more than a general notation of the child's age in years in a declaration that might be made several years after its birth.[14] Very few specific months of birth can be reconstructed from the surviving declarations of this type (N = 27). The Roman declarations, on the

[10] Schulz (1942); see also Sanders (1927).

[11] It was hailed as a move to defend the status of the free inhabitants of the empire: *HA Marc. Aurel.* 9.7–9; see Schulz (1942) 80–1 for interpretation.

[12] Schulz (1943) 63–4; the rest of his evidence, Schulz (1942) 78–9, all of it from Egypt, clearly shows that copies were requested, and provided, only in exceptional circumstances.

[13] Bagnall and Frier (1994) 27; the original sources are commented upon by Montevecchi (1988) 179–80.

[14] Montevecchi (1947); cf. Mertens (1958). Dated documents from 124–295 CE (most are from the 130s [N = 2], 140s [N = 2], and 150s [N = 6]).

other hand, are very exact, noting not only the day of the request for a copy, but also the day on which it was issued; and the copy of the original document contains the vital information in which we are interested: the precise day of the birth of the child.[15] Therefore, although the absolute numbers of these declarations that have survived (N = 23) are not much greater than the 'Graeco-Egyptian' types, the number of recorded months of birth that can be retrieved from them is much greater. The grand total from both sources, however, is still exiguous.

Table 2: Seasonal Distribution of Births in the Birth Declarations from Roman Egypt, *c.* 50–300 CE (N = 18)

J	F	M	A	M	J	J	A	S	O	N	D
2	0	2	1	1	2	2	5	2	0	0	1

Data source: Montevecchi (1927, 1928, 1988) plus additions

The known instances are spread over a period of three centuries, from various locales in Egypt. The pattern of distribution is indeed suggestive but, once again, some context provided by a larger statistical sample would be useful, especially in the light of what *seems* to be the aberrant behavior of the Egyptians in their birthing cycles ('unusual', that is, compared to other Mediterranean populations: see below). There is also no doubt that individuals almost certainly kept their own types of birth records as, for example, the fictitious freedman Trimalchio, the owner of vast latifundist estates in Italy and North Africa, who had his bailiffs maintain careful records of the births of the slaves born on his various plantation farms.[16] But domestic or household records of this type have not survived.

The question we have asked about the seasonal birthing cycle of Roman women is indeed partially answerable, however, since at least *some* evidence exists that is relevant to its resolution. Before advancing to the Roman case, however we should first try to understand something of the nature of the phenomenon of seasonality of birthing

[15] Montevecchi (1948). Documents dated from 62 to 242 CE (most are bunched in the 120s [N = 2], 130s [N = 2], and 140s [N = 6]). Cagnat (1927) is a useful discussion with facsimiles of some of the original texts.

[16] Petron., *Satyr.* 53 suggests that slave holders kept daily log books of births in their servile households, but see Friedländer (1891) 259 for some of the problems.

as it has emerged from numerous modern studies. Firstly, the problem of the seasonal phenomenon of birthing would not merit much attention on its own if the fluctuations were random in incidence or trivial in magnitude. Seasonal fluctuations in births, however, are quantitatively significant when compared to other temporal variations in human fertility. Even in very modern type populations, for example those of most western European countries, the United States, Canada, and Australia, the degree of seasonal variation—the measure of the amplitude between highs and lows (which are recognizable as peaks and troughs on a graph)—are considerable, sometimes varying between 10–30 per cent above or below the annual average for a given population.

Even more remarkable than the degree of this seasonal variability, however, is the *stability of the pattern* for any given population. For any given society, the seasonal patterns in human reproduction are very persistent over long periods of time.[17] Not only does a specific human population group seem to reproduce according to distinctive annual rhythms, it also continues to do so in that same pattern for extended periods of time. For example, the three-hundred year long series reconstructed by Wrigley and Schofield for England reveals a remarkable persistence in the birthing patterns over the whole period for which modern records are available for the analysis of past English populations (fig. 4). Although there is a long-term trend to the decline in the amplitude of the seasonal variations in birthing, from 48 per cent at the turn of the seventeenth century to 25 per cent by the early decades of the nineteenth, the general patterns of birthing, with highs in certain months of the year and lows in others, remained remarkably fixed, even into the decades after the Second World War.[18] The same characteristic can be seen in almost every other case where there exist good statistical data for long periods of time. In the Canadian case, for example, although the amplitude of the fluctuations in the seasonal patterns of births has tended to decrease somewhat with the advance of time, the same general pattern of seasonal births characterizes the whole period from the 1920s to the 1990s, during which time the basic demographic profile of the population has changed from rural to urban, from agricultural to indus-

[17] Lam and Miron (1991a) 53.

[18] Wrigley and Schofield, 'Short-Term Variations: Some Basic Patterns', ch. 8 of Wrigley and Schofield (1989) 285–355.

trial, and from premodern to modern in respect of educational opportunities, medical and health services, and communication technologies (fig. 5).[19] In modern societies, the seasonal variation of a naturally caused phenomenon like death has tended to become rather evenly distributed across the divisions of the year. But for these same societies, where one could presume that the artificial control of conception might have substantially altered the temporal incidence of births during an annual cycle, births are usually still distributed unevenly, sometimes very unevenly, throughout the year.

The problem facing the historian of Roman demography is a dual one: a technical problem of finding the means to discover the birthing cycle for Roman women and an interpretive problem of explaining the pattern. Obviously, the uncovering of the birthing pattern is not feasible by any direct means since (as has been pointed out above) birth records of any official sort have not survived in the quantities needed to produce a dependable series. But there does exist another large body of data which, with a little ingenuity and effort, can provide some of the requisite birth dates: the epitaphs on Roman tombstones. The funerary inscriptions of the city of Rome and of the various regional areas of the Italian peninsula are rich in the quality of their demographic data and, above all, in the large numbers of them that have been collated. It is the numeric or quantitative evidence relevant to the lives lived by the deceased recorded on these stones that are of the greatest interest for our inquiry. The 'pagan' or pre-Christian funerary epitaphs, as exemplified by the epitaph that follows, tended to have only one numeric quantity recorded on them—the length of life that had been lived by the deceased.

> Dis Manibus/Aequisiae Martinae/libertae benemeren/ti et coniugi v(ixit) a(nnis) xxvii,/m(ensibus) xi, d(iebus) xvi P(ublius) Aequisi/us Aprilis sibi et suis/po(s)te(risque?) eorum
>
> (CIL 6.11176: Rome)

With this information alone, it would *not* be possible to resolve the problem that we have set ourselves. The Christian revolution in attitudes towards death and dying, however, produced a new datum that Christians began to include on their funerary stones or plaques, and it is this new information that makes the necessary calculation

[19] Trovato and Odynak (1993), at 25–6.

possible. Because of their new and intense interest in the time *when* they died, and about the point in time at which they passed from this life to that of eternity, Christians tended to record the precise time (i.e., day) of death in their funerary epitaphs.[20] If Christian burial markers had only this precise quantitative datum on them, our problem would still not be resolvable. For a fairly substantial portion of the Christian memorials, however, an indirect means of calculating the birth date of the deceased is possible.[21] This can be done for a range of Christian burial markers that provide two precise numeric data in their epitaphs: the traditional notation of the length of the life that had been lived by the deceased and the new Christian datum of the precise time when he or she had died. A typical example is illustrated by the following inscription:

> M(arco) Aurelio Isidoro sive Acacio/filio carissimo qui vixit ann(is)/ xviii, m(ensibus) ii, d(iebus) xxiii, et M(arco) Aur(elio)/Commodiano Aspasio nepoti/dulcissimo qui vixit ann(is) viiii//men(se) uno, d(iebus) xxviii/M(arcus) Aur(elius) Isidorus et Herennia/Hermione infelicissimi parentes et avii fecerunt/d(e)p(ositus) Acaci filii d(ie) kal(endas) maias, item d(e)p(ositus) Aspasii nep(otis) d(ie) iii id(us) febr(uarias)
>
> (ICUR 10.26492: Rome: Coemeterium Pamphilii)

The requisite data must be reasonably precise and they must include (1) the date-at-death (the month and day), and (2) the precise length of the person's life in years and months and, in some cases, days. It is then a relatively simple task to subtract the precise length of life (2) from the fixed point in time of death of the deceased (1) to arrive at the date of that person's birth.[22] By using a large data set of Christian inscriptions originally assembled for a study of seasonality in the annual cycles of mortality for Rome and Italy, it was possible to cull from them the inscriptions that matched this profile and to begin our analysis.[23]

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[20] For the novel ideology of death, its origins, and how its impact can been seen in the funerary epitaphs of the Christians: Shaw (1996) 102–04.

[21] Much the same as the 'indirect' method was used to calculate the ages-at-marriage for Roman women: Shaw (1987) 39–40.

[22] One must caution that the method is only *relatively* simple, since the actual computation, which involves counting backwards in time, frequently across the December/January divide, with variable lengths of months, and with the Roman systems of counting, is in some ways rather complex and demands care.

[23] Shaw (1996) 105–06 figs. 1–2 for the chronological range and distribution of

By using this method, the dates of birth can be reconstructed for 1,447 persons, or about 32 per cent of the whole sample from which the figures for birth dates were derived. The persons whose birth dates could be reconstituted are divided almost equally in terms of gender (males, N = 753, or about 52 per cent; females, N = 694, or about 48 per cent of the whole).[24] When these birth dates are distributed by month through an annual cycle, the following patterns emerge. For the city of Rome the high point in births was located in the winter months of December and January, with secondary highs in May and August, and with troughs between these points (fig. 6). The pattern for the regions of Italy outside the metropolis reveals a rather different and more distinctive pattern (fig. 7). The months with the highest incidences of births are February and March; these are followed by a long 'depression' of very low rates of birth through the months of the spring and the summer. The incidence of births then resumes higher levels beginning in the period between September through November.

There are some significant points of difference between the birthing cycle typical of women from the city of Rome and the cycle typical of women from other regions of Italy that bear mention here. First of all, the amplitude of variation in the cycle is much greater for women outside the city of Rome. The average difference between the highs and lows for the Rome data set is about 50 points (index range *c.* 80–130) whereas in the Italian set of data the range is about 110 points (index range *c.* 60–170). The degree of seasonal variability in births for Roman Italy was therefore about twice the amplitude of the city of Rome. The difference results from the fact that

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the inscriptions, a description of the burial practices and social and cultural practices associated with them, and an analysis of the persons involved. There are a numbers of stereotypical objections that one encounters in the use of these data. One that is frequently asserted—that because the most significant available epigraphical data are restricted to the period of the fourth and fifth centuries, and to Christians, the resulting patterns are invalid for earlier Roman populations—is not particularly convincing. Given the propensity of birthing cycles to remain relatively fixed over very long periods of time, there is every reason to believe that the convergence of the ancient Roman data with the early modern Italian birthing cycles offers some indication of the probable continued presence of the same basic causes and patterns during the first to third centuries. The only discernable new factor that might have altered the general pattern was the possible effect of Lenten prohibitions (see below), the effects of which, however, were only discernable for the populations outside the metropolis of Rome.

[24] For a detailed description of the whole data-base from which these figures were derived, see Shaw (1996) 107.

the Rome set has secondary highs (in May and August) that tend to even out the overall seasonal distribution, muting the general effect of the midwinter highs. In short, the human population that was reproducing itself in the city of Rome behaved differently from that in the rest of Italy. The pattern of reproduction typically found in regions of Italy outside Rome suggests that these persons engaged in more intense and successful procreative activity (to put the matter politely) in the period between December and June, and much less so over the remaining months of the year. In Rome, on the other hand, the urban population maintained both this rural pattern, with intense cycles of reproductive behavior in the period between March and May, but with two additional sub-periods, one about August and the other about November. The general explanation must lay in part in the nature of the general environmental cycle that produced uniform and distinct periods of higher and lower reproductivity. In the urban setting of the metropolis of Rome, on the other hand, the underlying long wave of reproduction was broken by intrusive periods of higher levels of reproduction that were 'written over' the underlying annual cycle.

There is a final distinction between the two cycles that is worth noting. The difference becomes manifest when the two series are compared for the month of December. Whereas the month of December was a continued peak period of birthing numbers in the city of Rome, the data for the rest of Italy show signs of a rather significant decline. Whereas the month of December at Rome represents one of the highest periods of birthing (index = 109), for the rest of Italy the month of December is the nadir in the frequency of births, with well under half the frequency found for the city of Rome (index = 52). Furthermore, a glance at the graph for Italy (fig. 7) shows what is apparent from the statistics themselves: that the peculiar behavior that manifested itself in the month of December ran against the general trends for birthing in the late autumn and winter months, even for areas outside the city of Rome. When the data were first collected and analyzed, it seemed that this downward 'spiking' in the cycle might just be an odd effect of a small data sample. Yet when each subset of the data was checked (i.e., for different regions of North Italy, South Italy, and Sicily), the same characteristic pronounced 'dip' or downward turn in the data occurred at this same time in the year. The pattern is therefore not just a quirk resulting from a small sample. The sudden downward turn in

births in December must be backdated to the sexual reproductive behavior that produced it, sometime between the end of February and the end of March of each year. In fact, both of the Roman and the Italian birth cycles must ultimately be correlated with explanations of the sexual reproductive behavior itself, rather than the resulting profusion of births some nine months, or so, later.[25]

Another possible means of counter-checking the seasonality of birthing cycles is to use the proxy data provided by the seasonal distribution of infant mortality for imperial Rome and Italy. *Ex hypothesi*, the distribution of infant mortality should provide a good source of proxy data. It is already known from historical studies of the demography of the premodern Mediterranean that infants were not born in numbers that were evenly distributed throughout the year, but rather that births were bunched quite markedly in periods of greater and lesser frequency. This was also true of the city of Rome and regional Italy in the period of the Roman Empire. There were certain months of the year when many more infants were born as compared to other parts of the year. Given the deleterious effects of premodern sanitary and alimentary conditions, infant mortality rates in general must have been very high. One would further expect that infants who were at risk at the time of birth or soon after birth would have died in the month in which they were born.[26] Generally speaking, studies on the premodern Mediterranean and the early modern Italian data concerning the correlation between seasonal birthing cycles and the seasonal cycles of infant mortality confirm that the match between the two is usually close.

Where infant and child mortality is concerned, most circum-Mediterranean countries in their premodern phases of development have a striking similarity of seasonality in infant mortality, with maxima in the period between October to February, with a pronounced maximum in December–January, and with very low mortality rates, by comparison, in the period from May to November. The great exception to these mediaeval and early-modern Mediterranean seasonal

[25] As historical demographers have realized, although the causes of the seasonal nature of this 'creative' part of human behavior must exist, these causes are very difficult to discern: Lam and Miron (1991b); Lam, Miron and Riley (1994); Roenneberg and Aschoff (1990); Rojansky, Brzezinski and Schenker (1992); Udry and Morris (1967).

[26] This assumes the uniform impact of the causes of death on all infants throughout the year, an assumption which requires some qualification, see Knodel (1983).

cycles—an important exception that must be noted in the context of studies of ancient Roman demography—is that of Egypt, where the patterns of infant birthing and mortality are precisely the reverse of the norm for the rest of the Mediterranean. The Egyptian data are characterized by highs of infant and child mortality in the months from May to September, with pronounced maxima in June and July, and with lows in the rest of the year, the minima being concentrated in the months of February and March.[27] What is important for us, however, is that for the premodern population structures of Italy, the general Mediterranean patterns hold true. The birthing cycle is the same, as is the infant (< 5 years) seasonal mortality curve: the maximum death rates are in January to February with the minimum in September.[28] The reason why the maxima in both demographic patterns are so closely correlated is manifest. It is in the same months in which infants are born that they are most vulnerable to lethal disease vectors in the environment. Therefore the months in which by far the heaviest infant mortality occurs will tend to be the same months in which more infants are being born.[29]

The factor of the seasonality of infant and child mortality also signals a caution about the simple acceptance of seasonal distribution of mortality. There is no doubt that seasonal mortality is very pronounced for this particular cohort, which I shall define, for purposes of analytical convenience, as formed of two distinct groups: (a) children under one year age, and (b) those between one and five years of age. The acquisition of data on the mortality of either group for any large premodern city is difficult. Usually, the records do not exist and the rates have either to be postulated or to be reconstructed by indirect means.[30] If one only had the statistics for infant and child mortality for ancient Rome, it would seem that measuring the seasonality of infant mortality would be a rather straightforward matter of giving the proportions dying in each month of the year. This calculation is not so simple, however, because of another great divide that separates premodern from modern populations, and which is

[27] Biraben and Henry (1957) 634–7; Bchir (1966); Tabutin (1974) 207–10.
[28] Di Comite (1974) 193.
[29] There is some 'drag' effect that is apparent in some cycles, where there is a 'gap' of a month or so between the mortality trends and the reception of abandoned infants by institutions: Da Molin (1983).
[30] Landers and Mouzas (1988) 62–6.

the precise object of our inquiry here: the seasonal rhythms in the production of children.

In most of the premodern populations of the Mediterranean, births have had their own distinctive seasonal cycles.[31] High seasonal maxima of infant deaths in the main winter months (December–February) coincide with the period of the highest points in the birthing cycle. The simple fact is that there were far more infants being born in these months, and it was the newborns that were most at risk. As has been pointed out in the Italian case, most of the statistics that produce the maxima for children under five years of age are accounted for by the deaths of infants in their first months of life. Analysis of the seasonality of infant mortality for various regions of Italy in the early 1950s also reveals regional peculiarities. Districts in the north had pronounced highs in infant mortality in the winter months along with secondary highs in mid-summer, whereas those in the deep south tended to have single highs in mid-summer. For the central regions of the peninsula of most relevance to our case, including the region of Lazio, infant mortality tended to have double highs and double lows: high mortality rates in the winter months of December and January, and then again in the mid-summer months of June to August, with intervening lows in the spring months of April and May and the fall months of October and November.[32] The principal causes of death were respiratory infections in the winter months, and gastro-intestinal infections in the summer months. Studies of infant and child mortality for Italy in the nineteenth century confirm the probable long-term persistence of these patterns. Infant mortality was most pronounced in the first month of life, and this heavy infant mortality displayed the greatest seasonal variation from region to region. For central Italy, including Lazio, it was most pronounced in the winter months of December to February, with a secondary high in the autumn period of September to November.[33]

Another means of cross-checking the ancient data with premodern Italian conditions would be to consider seasonal patterns in the abandonment of infants.[34] Most infants were abandoned by their

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[31] Tabutin (1974), esp. ch. 4, 'Variations saisonnières de la mortalité aux jeunes ages' (195–222).

[32] Biraben and Henry (1957), at 624 ff. and the graphics 624–5.

[33] Breschi and Livi Bacci (1986) 15–18.

[34] Da Molin (1994) contains the best recent discussion and bibliography.

families at or close to their birth, for a number of reasons including the stigma of illegitimacy and the pressures of poverty weighing upon poor families. The statistics on the months of the year during which most infants were abandoned should offer a reasonable correlation with the actual birthing cycle. Analyses of the records kept by religious foundations that served as havens for abandoned infants through the seventeenth and eighteenth centuries yield a reasonably consistent picture (fig. 8). The early months of the new year, the high winter months of January, February, and March are the commonest months for the reception of abandoned infants. There might be a small retarding factor involved, but it is not likely to be very great. This would indicate that the months from December onward would be the months of the highest rates of birthing.[35] Detailed studies of abandoned infants received at Camerino, Naples, Perugia, and Rome in the early eighteenth century all show a close convergence of the months of the year in which the greatest numbers of abandoned children were received with the months of the highest numbers of births.[36]

It would seem that these same patterns would also have been true of late imperial Rome, but there is, alas, no way of proving it to be so by recourse to the proxy data—for this is an instance where we encounter another, and this time truly intractable problem with the ancient Roman data. It is possible to reconstruct a probable pattern of seasonal mortality for infants and children in ancient Rome (fig. 9); but a glance at the comparative data for early modern Italy (figs. 10–12) immediately reveals the absolute shortfall in a critical sector of the Roman data. The Roman statistics are generally good (i.e., available in reasonable numbers) for infants that died when they were several months old or older; and the figures become even more abundant for those several years old (say, between two and six years

[35] Povolo (1982), esp. 655, 'Tabella II: Stagionalità dell'ingresso dei bambini negli ospizi di Venezia, Padova, Vicenza e dei battezzati delle città di Venezia e Vicenza'. For general studies of the problem, see Da Molin (1982); and Kertzer (1987), for an analysis of the cultural and political factors that conduced to higher levels of abandonment in the nineteenth century.

[36] Da Molin (1983) 109 (Naples and Perugia) and 110 (Rome and Camerino) for the seasonal distributions (highest in March to May); there is a slight 'retarding' effect of a month or so, but as Da Molin notes, the two cycles follow each other so closely in sequence that the one is clearly feeding directly into the other (111).

of age). Analysis of the early modern Italian data, however, has shown that the *critical* shift in seasonal mortality takes place between infants up to their first month after birth (i.e., who are only a few weeks old) and all the rest (whether the infants are several months or several years old does not seem to matter). That is to say, infants who survived the first month of life quickly assumed a pattern of seasonal mortality that was like that of the general population of which they were a part (fig. 11–12).[37] Those infants who died close to or at the time of their birth, however, were part of a different cycle of death that was directly related to the circumstances of their birth. It is this cycle of infant mortality, and no other, that matches the birthing cycle rather closely; but it is precisely these data on neonatal deaths that are entirely absent from the ancient Roman data (fig. 9). The reason must be that the deaths of infants at or near the time of birth, perhaps those who died before they were involved in their naming ceremonial, were regarded as somehow malign and taboo, a tragic event that provoked a commensurate 'denial' of the child's death.[38] Hence even those Roman populations that were most disposed to the commemoration of the young, even the very young, in funerary practice—the urban-centered populations of the empire and Christians—did not commemorate infant mortality at this basic level by means of inscribed grave markers. Therefore, none of the records that we need were ever produced.

Birth rates in the premodern demographic regimes of circum-Mediterranean countries were generally high in January and February and have declined to minima in June and July, before they began to ascend again in September and October. This birthing curve seems to be true of most Mediterranean countries of the premodern era (one must note, again, the significant exception of Egypt).[39] It can be found in the indigenous populations of the modern state of Israel at the far eastern periphery of the Mediterranean.[40] This Mediterranean pattern is distinguished from the birthing patterns typical of northwestern European countries, one which is significantly different than those found in zones to the south.[41] The reasons for

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37 Bellettini and Samoggia (1983), esp. 203–04.
38 Néraudau (1984) 379–84; cf. Shaw (1991) 77; Montanini (1991) 98–102.
39 Marcoux (1970) 173–93 (with replies and discussion 194–213).
40 Guptill *et al.* (1990), esp. 217 table 3, fig. 1; and 218 fig. 2.
41 Vallin and Le Bras (1970) 1268–73: escalating to a high in May, then a con-

this peculiar birthing curve in the premodern Mediterranean have been much debated, since no convenient explanation seems sufficient. No single set of factors seems to account for the pattern—certainly no peculiar economic cycle, since the basic premodern Mediterranean birthing pattern is the same for pastoral and agricultural groups, for herders and peasants, for urban and rural populations, and for those whose income depended on craft manufactories as much as for those who depended on the sea for their livelihood. Nor do peculiar religious beliefs seem to explain much, since the same basic pattern is found amongst cultural groups that are differentiated by their distinctive Christian beliefs; and both Muslim and Christian cultural areas in the Mediterranean display much the same birthing cycles.[42] That this peculiar distribution of births was fully characteristic of the early modern Mediterranean populations, and that such a seasonality of birthing probably also characterized the population of Rome that we are studying here, must also be kept in mind when the seasonality of infant and child mortality is being analyzed.

Explanations for these strongly persistent patterns of reproduction in premodern human populations are therefore rather complex.[43] Because of the consistent apparent paradox between 'most preferred' months when prospective parents would wish children to be born and the patterns of birth, one can almost certainly rule out the effects of conscious family planning.[44] Other causes, such as the determinate impact of economic and social class, and related factors, have been canvassed, but without much conviction.[45] As more data have been accumulated and studies of them have been made, it has become clear that the patterns of human reproduction are configured in large

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tinual decline to a low in August, a sudden upward 'spike' in September, followed by a further decline to November.

[42] Léridon (1973) 55–84.

[43] Lam and Miron (1991a) 67–75; and (1993), where they canvass the most frequently studied causes (weather cycles, agricultural cycles, economic variables, holiday periods, marriage rates) and point out the difficulties with each of them (i.e., they do not explain the striking exceptional cases).

[44] Rodgers and Udry (1988) 171–4: hardly a convincing text. They relapse onto the 'misinformed reproducer hypothesis,' which seems equally unlikely as an explanation. I have retained the paradox, since the same phenomenon appears to be true of other populations.

[45] James (1971) 309–20, who begins with the not very controversial observation that a major cause of seasonality of birthing in the U.K. 'appears to be seasonality in sexual activity.' James tried to demonstrate a greater seasonality of illegitimate and 'upper-class' births. Such a fine distinction in class differences, however, is beyond our current state of evidence to decide in the Roman case.

scale meta-patterns that must reflect global responses of human populations to similar environmental effects. For example, a detailed and large-scale study for Scotland has revealed seasonal patterns that match those for Canada and the northern United States, but which are quite unlike those of the more southerly latitudes in the United States where the seasonal cycle of births is almost completely the opposite.[46] Demographers have therefore been driven increasingly to consider certain universal environmental factors, such as photoperiod effect, to explain these fundamental long wave patterns in human reproductive behavior.[47]

Imprinted over top of these basic long wave oscillations, however, there sometimes appear rather odd disruptions of the basic patterns—unusual breaks in the cycles that are almost certainly to be correlated with cultural factors. One example will have to suffice. In the general long-wave of seasonality of birthing that characterizes the populations of Canada, the northern states of the United States, and the northern countries of western Europe, the long oscillating wave of birthing (with highs in March to May descending to lows in July to November) is suddenly and dramatically disturbed by an odd and very distinctive high that spikes upwards out of the nadir of the curve in the period of September and October.[48] By back dating the results to the time of the cause of this sudden upsurge in birthing, it is manifest that the behavior must be related to the effects of the preceding Christmas holiday season.

This observation leads us to reconsider the cause behind the singularly odd downward 'spiking' in the month of December found in the regional Italian statistics for the Roman period (fig. 7). The peculiarity, however, is also found in many other early modern European birthing cycles and is the manifest effect of the previous Lenten 'prohibitions' as practiced by Christians—not only of marriage but also of consistent sexual activity that would be observable in reproduction late in the year following.[49] What this seems to show is a peculiar

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[46] Russell, Douglas and Allan (1993) 362–7; results are presented in 634 fig. 2 (a modular wave with highs in March–April–May then a constant decline into a summer tough, with the single unusual spiking high in October, then the resumption of the decline to a nadir in November).

[47] Russell, Douglas and Allan (1993) 364–5.

[48] Trovato and Odynak (1993), 26 fig. 2 and 30–33 (figs. 5–6); Lam and Miron (1991a) 59 figs. A–D.

[49] On the general history of Lenten prohibitions and their effects on seasonal

effect of Christianity on the birthing cycles of Roman women that is perceptible in the regions of Italy outside Rome, but not in Rome itself. This observation, if true, then demands other additional hypotheses, as for example ones on the variable effects of religious observance that were more muted in the urban environment, which would themselves demand further investigation and testing. If this observation is borne out by other studies, however, it would be a significant additional result of this study that would connect something as unusual as the historical demography of birthing with the cultural geography of practices as profound and widespread as those of linked with early Christian behavior and belief.

In conclusion, one can reasonably postulate the presence over a very long period of time of a birthing, and procreative, cycle involving Roman women that was characterized by a long-wave cycle of much higher numbers of births in the late autumn and winter months, and much lower rates in the intervening periods. This pattern of birthing suggests more intensive cycles of procreative activity in the months between April and June of each year. It must be emphasized, however, that what is important in this investigation is not the specification of individual months of high birth rates, but rather *the coherence of the general cycle* that characterized the population as a whole. In presenting the initial results of this brief investigation into the seasonal birthing cycles of women in Rome and Italy, there are, of course, a number of caveats that should be expressed.[50] However tentative the reconstruction of these birthing cycles must be in the present stage of our research, one must remember that the recov-

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cycles of marriage, see Shaw (1997) 70–1; for studies detailing the effects on premodern birthing cycles see Cardamone (1975) 233 tab. 5; Prioux (1988) 590 fig. 1, 596 fig. 2; Léridon (1973) 66 fig. B.12; often it is not clear in many cases (e.g., our graphics for early modern southern Italy) where the birthing cycles have been reconstructed from baptismal records since these do not reveal the low incidence of births as a result of Lenten prohibitions as clearly as do raw birth records.

[50] Some of the objections, however, are only apparent. One that was frequently voiced linked these results with an argument of mine concerning the seasonal marriage cycle of Roman women where it was demonstrated that the most frequent period of marriage (for these same Roman women) was the winter acme of December to February (Shaw (1997)). The objection is that this should produce a birthing peak in September to October. This is a simple misunderstanding of the problem. The objection *would* be true if *all* births in a human population were constituted only of the *first* births resulting from marriages. The seasonality of birthing indexes presented in this paper, however, measure the sum of *all* births of all infants produced over all women in a given demographic system—and it is certain that Roman women were giving birth to many infants other than their first child.

ery of similar results for many European cities during the early modern period is often a difficult task. Even in the case of a large city like London for the sixteenth to eighteenth centuries, direct data for these cycles do not exist and the seasonal birthing patterns often have to be reconstructed, as in many of the studies of early modern Italian communities, *indirectly* from baptismal records (fig. 2).[51] In this sense, then, the historical demographer of imperial Rome, who is employing a different method of reconstruction, is no worse off than many of his or her modern counterparts. Even if the patterns suggested here are rather general, and therefore lack the evidence on the fine oscillations that would necessarily characterize each regional and temporal sample, the type of data to which modern historical demographers have access, we have nevertheless provided a starting point—a basis from which future debates and discussions of the problem might reasonably begin to contribute a new aspect to the demographic history of Roman society.

[51] Wrigley and Schofield (1989) 289–91, discuss the main problems; see Landers and Mouzas (1988) 62–3; and Berry and Schofield (1971) *passim*; for some premodern Italian communities, see Ligresti (1984) 80 tabella 18: 'Movimento mensile dei battesimi (%),' for Mascalucia, Trecastagni, Viagrande and S. Gregorio, 1750–1800; 114 tabella 23: 'Indici di stagionalità dei battesimi: Leonforte' (Sicily); 136 tabella 37: Movimento mensile dei battesimi (valori assoluti e percentuali); 137 fig. 5: 'Leonforte: Battesimi. Movimento mensile'; and 167 tabella 52: 'Battesimi, sepolture, matrimoni. Movimento mensile.'

Note: Indexes of Seasonality

The absolute quantitative data concerning a specific demographic factor for any given population group (for example, regional, temporal, or ethnic) will usually be very different from those for any other such group. One of the primary desiderata of historical demographers in tracking the seasonality of a specific demographic phenomenon (for example, birth, marriage, or death) in a given population is to be able to compare the results for that group with the seasonal cycles typical of other populations. To be able to effect such comparisons directly, historical demographers reduce the fixed and absolute numbers for any given data set to so-called 'indexes' of seasonality. The concept is relatively simple. The method reduces the aggregate demographic statistics for any given group to their annual average which is then equated to the index number of '100.' The extent to which the figures for any given month (for example, those on the frequency of births) exceed this average, they have an index that is above 100 (e.g., 132). The extent to which they fall below this average gives them a negative index number (e.g., 78). The formula used to calculate the running index numbers for a given year is:

$$\text{Index number} = \frac{Ti/T}{Ni/365.25} \times 100$$

Where: Ti = the number of demographic events (births, marriages, deaths) in that month

T = the total number of such demographic events in that year

Ni = the number of days in the month for which the index is being calculated

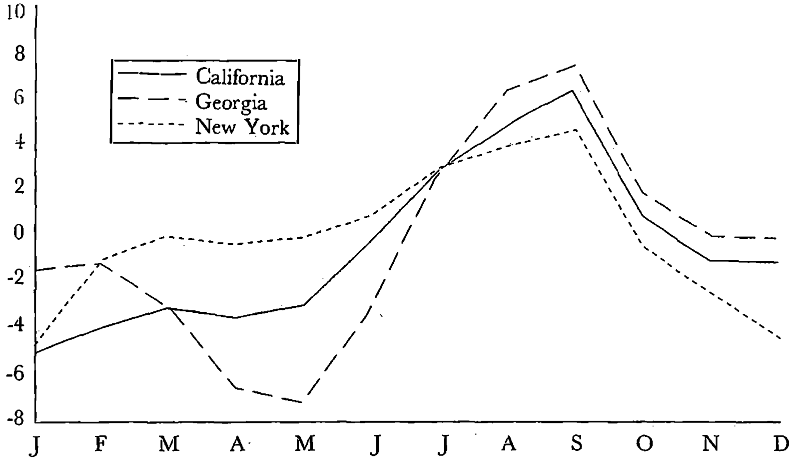

Figure 1: Seasonality of births: United States, 1948–1986
Source: Lam and Miron (1991a), p. 59, fig. 3

Figure 2: Seasonality of births: Bari and Foggia, 1816–1820
Source: Assante (1974), p. 126, grafici 10–11

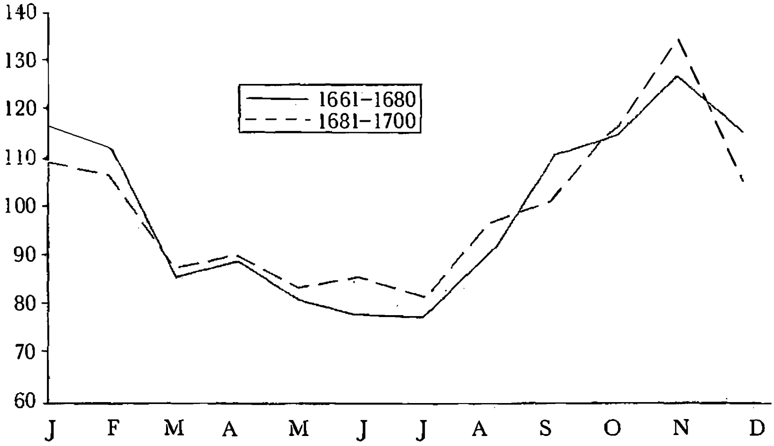

Figure 3: Seasonality of births: Bitonto, 1661–1700
Source: Cardamone (1975), p. 233, tabella 5

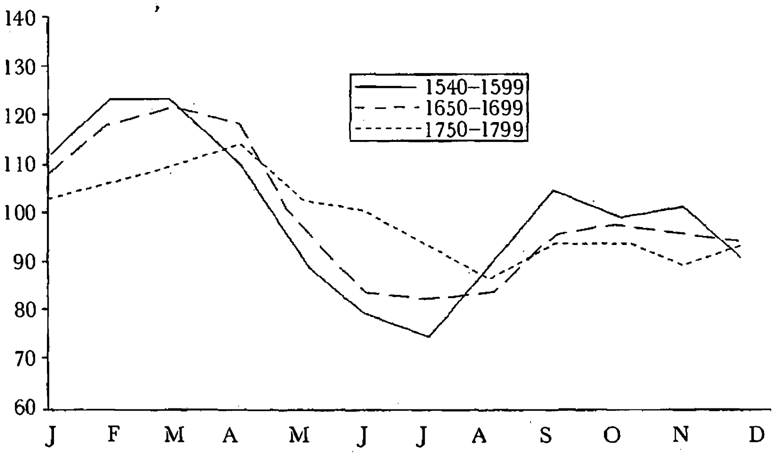

Figure 4: Seasonality of births: England, 1540–1800
Source: Wrigley and Schofield (1989), p. 288, fig. 8.1

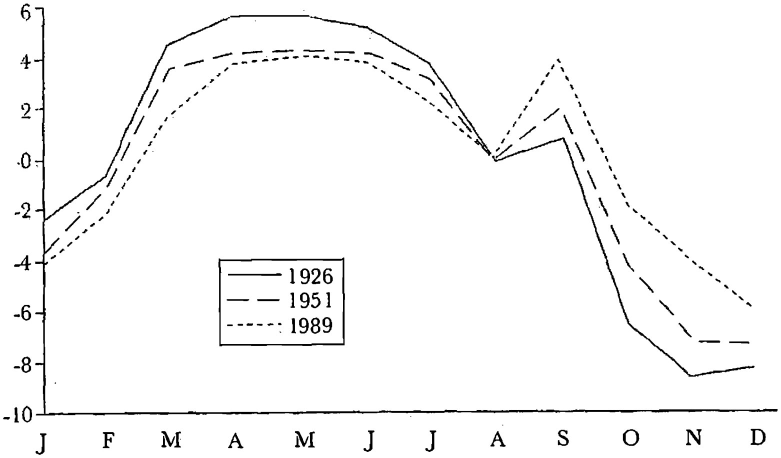

Figure 5: Seasonality of births: Canada, 1926–1989
Source: Trovato and Odynak (1993), p. 26, fig. 2

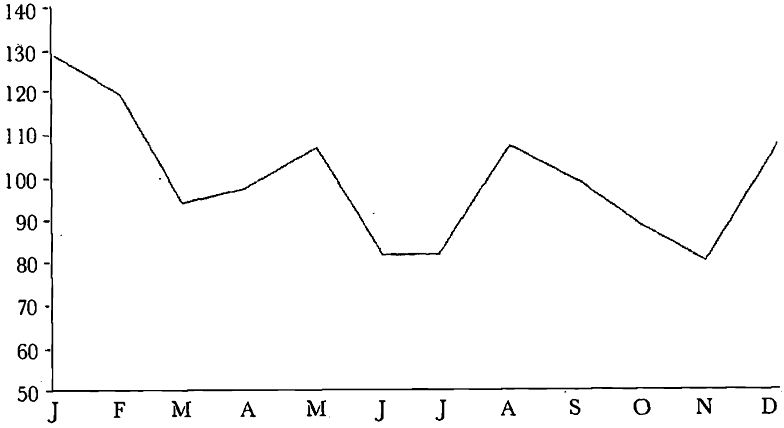

Figure 6: Seasonality of births: City of Rome, *c.* 4th–6th centuries CE (N = 1247)
Source: Shaw (1996), pp. 135–36

Figure 7: Seasonality of births: Roman Italy, *c*. 4th–6th centuries CE (N = 225)
Source: Shaw (1996), p. 136

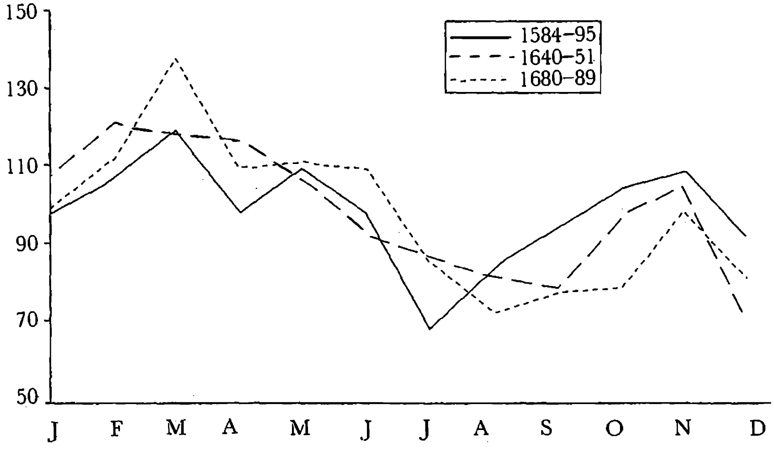

Figure 8: Seasonality of infant exposure: early modern Perugia (Italy)
Source: Calzola (1994), p. 33, tabella 2

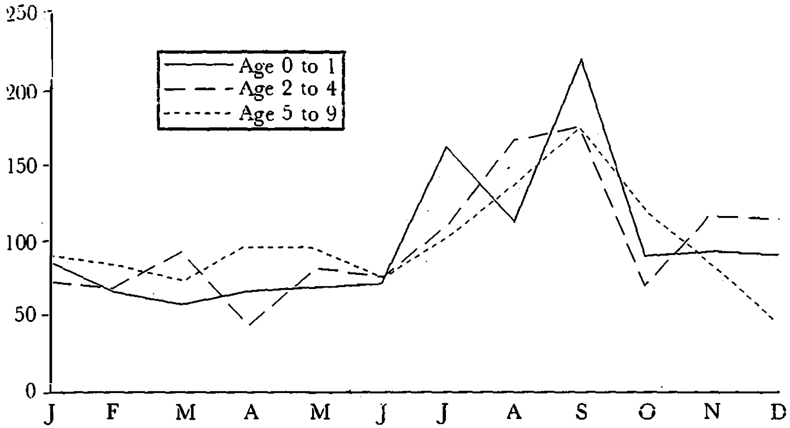

Figure 9: Seasonal mortality: City of Rome: Infants and children, *c.* 4th–6th centuries CE (N = 706)
Source: Shaw (1996), p. 118, fig. 9

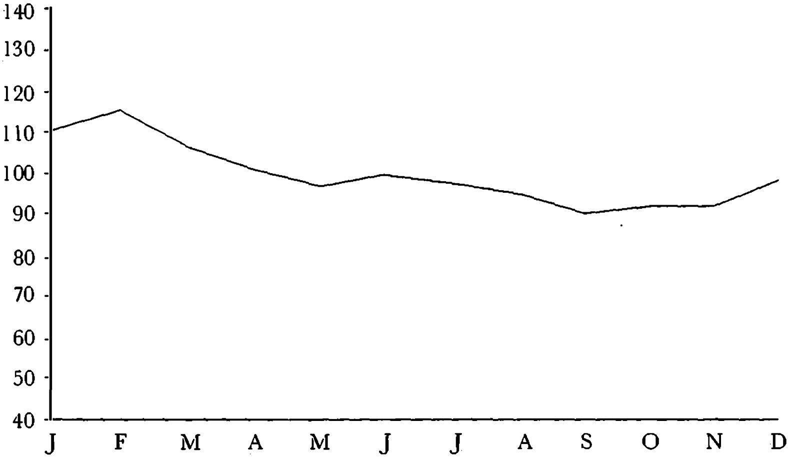

Figure 10: Seasonality of birth mortality, ages 0–5: Italy, 1958–1967
Source: Di Comite (1974), p. 190, tavola 91

Figure 11: Seasonality mortality: Infants and children: Italy, 1869
Source: Ferrari-Livi Bacci (1985), p. 280, tabella 5

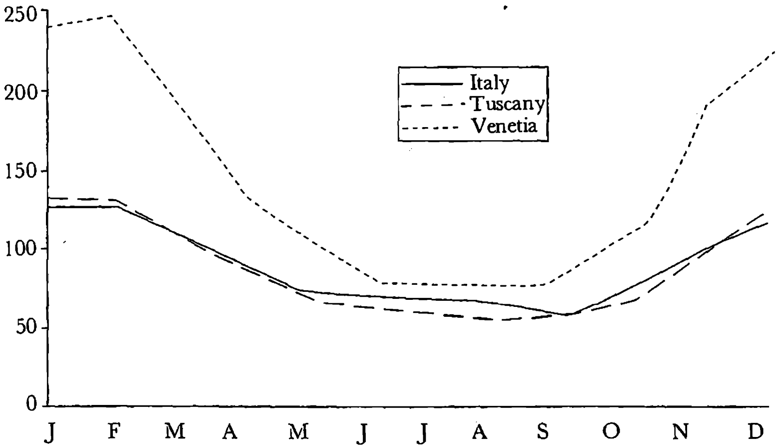

Figure 12: Seasonality of mortality: First month of life: Italy, 1872–1879
Source: Breschi-Livi Bacci (1986), p. 23, tabella 5

CHAPTER THREE

RECRUITMENT AND THE SIZE OF THE ROMAN POPULATION FROM THE THIRD TO THE FIRST CENTURY BCE*

Elio Lo Cascio

The two alternative explanations of the Augustan census figures advanced by Beloch and by Frank[1] do not only imply two different evaluations of the size of the Italian free population at the beginning of the Empire, but also two conflicting ways of envisioning the demographic development of the Republican age and of evaluating quantitatively the major phenomena which seem to characterise it: the difficulties of the peasant farmers in the second century BCE, the increase in the number of slaves and slave-staffed villas, the growth of the Italian towns and especially of Rome, the impact of the wars of conquest and of the social and the civil wars on conscription. That means that by choosing one of the two alternative explanations we are obliged to follow it all the way: their acceptability in fact depends on their internal consistency, and it is not possible to accept either of them just partially.

Thus, if one adopts Beloch's solution, one has to accept: 1) that the free population of Italy was collapsing; 2) that there was a huge influx of slaves, who came to constitute at least 40 per cent of the total population of Italy and perhaps more,[2] and, conversely, that

* I am most grateful to my friend Walter Scheidel for his help in improving both the argument of this paper and its presentation. Of course, this does not mean that he agrees with any of it. Needless to say, all remaining faults are mine.

[1] According to Beloch (1886) ch. 8.6, the Augustan census figures refer to the whole citizen population of both sexes and every age (with the exclusion of infants); according to Frank (1924), they refer, as the Republican ones, just to adult males. The most detailed and fully argued restatement of Beloch's explanation is Brunt (1987), introduction and ch. 9; the most authoritative follower of Frank is Jones (1948); on the controversy see now Lo Cascio (1994a, b) and (forthcoming a) ch. I. By now, Beloch's solution has been accepted by most scholars and can be considered as the current orthodoxy regarding the size and development of the population of Roman Italy.

[2] See Beloch (1886) 436, and Brunt (1987) 124 and passim, who arrive at the

the rate of manumission was not as substantial as commonly supposed; 3) that the percentage of the Italian population living in towns must have been very. high in comparison with other preindustrial scenarios; 4) that the percentage of citizen population living in the Po valley must have been comparatively much lower than the citizen population living in Peninsular Italy,[3] 5) that the burden of military service was tremendously heavy for long periods.[4] With Frank's solution, one has to accept: 1) that the free population of Italy was increasing and that it was approaching the level reached in the seventeenth or eighteenth centuries; 2) that slaves made up much less than thirty per cent of the total population at the time of Augustus[5] and that the rate of manumission was very considerable; 3) that the urban population of Italy was high, but not necessarily out of line with other preindustrial populations; 4) that the percentage of citizens living in Northern Italy must have been substantial, and not much lower than the citizen population living in Peninsular Italy, as was the case in early modern Italy when the northern regions

identical estimate of forty per cent starting from different absolute numbers for the free and slave population; see also, for instance, Hopkins (1978) 68–9, table 1.2. Scheidel points out to me that the assumption that forty per cent of the Italian population were slaves does not logically flow from a particular interpretation of the census figures; for the possibility that the slave population of Roman Italy was smaller than commonly suggested, see Scheidel (1999d). I agree that there is no independent basis for estimating the number of slaves (and that is the reason why I do not hazard any guesstimate). However, the point I want to make is different, namely, that either of the possible explanations of the Augustan census figures entails a set of hypotheses that must be mutually consistent. In my view, Beloch's solution obliges us to reckon with a large proportion of slaves: it is only by putting the slave population of Italy at a level high enough to raise the total population in 28 BCE considerably above 4 million that it becomes possible to account for the survival of the citizenry, given the implied extent of urbanisation, and, thus, the implied high proportion of the population who was not engaged in agriculture.

[3] Jongman (1988) 65 ff.

[4] The most detailed and most consistent presentation of all the conclusions stemming from the acceptance of Beloch's interpretation of the Augustan census figures has been given by Brunt (1987).

[5] Unless one wants to put the total population of Italy in the Augustan age at an impossibly high level: see Lo Cascio (1994b) 111; Schiavone (1996) 242, criticizes my attempt to challenge the orthodox view, by arguing that to put the Italian citizen population at ten to twelve million would not leave enough room for the substantive number of slaves (not less than thirty per cent) we have to suppose to have been present in the Italian peninsula, but note the circularity of this argument; and see also Gabba (1996) 332, who notices the serious implications of interpreting the Augustan census figures as referring just to the adult males for the whole issue of the so-called 'slave mode of production' in Roman Italy; see also Tarpin (1998) 32.

accounted for a little more than half of the total population of Italy (excluding the islands);[6] 5) that the burden of military service was again high, but comparable to that experienced in other preindustrial states.

That the two explanations propose quite conflicting scenarios, and that it is not legitimate to accept a mixture of them is best shown by Morley's recent attempt to argue that the most important factor preventing the growth of the Italian population during the two centuries from 225 to 28 was the growth of the population of the capital, achieved by continuous immigration of considerable proportions from rural Italy, given the inability of the population of a big preindustrial city like Rome fully to reproduce itself.[7] In this scenario the other reasons put forward for assuming the collapse of the rural population of Italy simply disappear as major factors of the demographic development of Italy. Morley maintains that even a substantial natural increase of the Italian free population during the last two centuries of the Republic would have been turned into its opposite by the growth of the population of Rome, itself due only to continuous immigration, given the likely negative rate of natural increase suggested by comparative evidence.[8] The problem with this explanation is that, even if we were to accept all the assumptions made, in numerical terms, by Morley (some of which are very extreme),[9]

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[6] Beloch (1937–61); Cipolla (1965); Bellettini (1987); now Del Panta *et al.* (1996) 79–80 and table 1; see also my comments on what Jongman (1988) 66, has maintained on this issue, in Lo Cascio (1994b) 108–9.

[7] Morley (1996) ch. 2.

[8] The obvious comparison is with modern London, for which Morley (1996) quotes Wrigley (1967) and Finlay and Shearer (1986); see also Landers (1987, 1993). The idea that size of towns and natural population increase were inversely correlated in preindustrial Europe is an old assumption, going back at least to Süssmilch: see Sharlin (1978) 126–7; De Vries (1984) 179–80. Morley (1996) is mostly based on Jongman (1990). I discuss this whole issue in Lo Cascio (2000a, forthcoming c).

[9] Morley assumes that the demographic development of imperial Rome is similar to that of modern London between the sixteenth or seventeenth and the early nineteenth centuries, since hygienic and sanitary conditions were in both cases very poor; he also assumes that the data for London suggest that deaths must have strongly outnumbered births in imperial Rome as well. The first assumption is not necessarily true, since the comparison cannot be mechanical: for instance the impact of a regular and substantial supply of water or of the widespread use of baths on the general sanitary conditions of a big preindustrial city has been debated (see in particular Scobie (1986) and Laurence (1997)), but on the whole it cannot be thought to have been unimportant as a peculiar feature of Rome. The second assumption is not necessarily true, since during a period of strong immigration, migrants into the city had, as is often the case, lower life-expectancy or belonged to age-classes

the very figures he arrives at do not seem to be acceptable. Drawing on the guesstimates of Keith Hopkins,[10] Morley assumes that the population of Rome was 200,000 (150,000 free and 50,000 slaves) in 225, and 850,000–1,000,000 in 28. The population of Rome in 28 was composed of what he calls the 'core population' of the permanently resident citizens entitled to the dole and their families,[11] estimated at 520,000, and of freedmen, soldiers, foreigners, the élite (estimated all together at 240,000), slaves (100,000–200,000), and finally of the recent 'migrants'. He then puts at one per cent the annual rate of natural decrease among the 'core population' (now set at 550,000) and at two per cent the rate of natural decrease among the more recent 'migrants' (put at 100,000), and calculates that in order to support the growth of the population of Rome, counteracting the high rate of natural decrease, 7,000 immigrants would have been required every year. The next step is to posit a decrease of the whole free population of Italy between 225 and 28 from 4,500,000 to 4,000,000. In his view, this decrease must have been the direct result of the demands of Rome on the population of Italy, on average 7,000 young adults per year: these demands offset the possible increase of the Italian population at large. Morley estimates that 7,000 migrants, given their probable age, were the survivors of an original annual cohort of 15,000 and that 'in a population of 4.5 million increasing at a rate of 0.5 per cent p.a., 15,000 births represent two-thirds of the total natural increase siphoned off to maintain the city of Rome instead of swelling the Italian population. A population whose rate of increase was less than 0.33 per

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with a lower life-expectancy and that will have raised mortality, just as in London: in other words, excess mortality does not necessarily imply a much higher age-specific mortality: see, for London, Sharlin (1978), opposing the 'model of urban migration' to the 'model of urban natural decrease' (I am not persuaded by the objections of Finlay (1981), for the reasons given by Sharlin (1981)). I develop this argument in Lo Cascio (2000a, forthcoming c). Nor am I convinced by the way in which Paine and Storey (1997) interpret the data of the funerary inscriptions in Rome as showing a real 'catastrophic mortality', instead of a biassed representation of funerary commemorative practices.

[10] Hopkins (1978) 68–9 table 1.2.

[11] This idea of a distinction between the more stable population of resident citizens and the 'migrants', with different demographic features, which goes back to Sharlin (1978), and has been exploited by Ringrose (1983), with reference to Madrid, has its own merits, even if I believe that it could be better exploited in the case of Rome of the early empire, after the limiting of the *plebs frumentaria* by Augustus: see Lo Cascio (1997) 47 ff., (2000a, forthcoming c).

cent p.a. would start to decline under the pressure of this emigration'. His conclusion is that 'if this constant drain is taken into account, a rise in population from 4.5 million to 10 millon (or rather 9.4 million, excluding the population of Rome) in two centuries required a rate of natural increase of over 6 per thousand per annum' and that 'a rate of increase among the rural free population of 3 per thousand per annum would have resulted in a slight decline over two centuries from 4.5 to 4 million': as a consequence, he holds, 'Frank's interpretation of the Augustan census figures may . . . be rejected on the grounds of demographic plausibility'.[12]

It seems to me that this reconstruction is invalidated by several shortcomings. First of all, it is possible to show that employing Morley's own assumptions about the rates, natural increase in Italy outside Rome could easily have supported the required levels of migration to the capital and enabled the non-metropolitan free population to grow at the same time.[13] The mean rates of population change posited by Morley can be interpreted in two ways. In one scenario, the free population of Rome grew at a constant annual rate of slightly over 0.7 per cent from 150,000 to 650,000. Thus, in 225 BCE the metropolis and the rest of Italy held 150,000 and 4,350,000 free people, respectively. Only one free person out of thirty resided in the capital. That year, the non-metropolitan population increased by 0.3 per cent, or 13,050 individuals.[14] At the same time, Rome would have experienced both a decrease by one per cent, or 1,500 persons, and net growth by 0.7 per cent, or 1,050 persons, requiring a total of 2,550 immigrants. These accounted for one-fifth of total growth outside the city; four-fifths of the additional free people stayed in Italy (or at the very least did not settle in Rome on a permanent basis). This annual surplus would have gradually shrunk

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[12] Morley (1996) 50.

[13] This, at any rate, is true in the absence of other attrition factors, which are not part of Morley's model: this rebuttal deals with his claim that the 'demographic burden' alone could have been sufficient to reduce the total free population from 4.5 to 4 million.

[14] It is confusing to state, as Morley does, that the *total* population of Italy would have grown at 0.3 per cent a year if the 'core' population of Roman citizens in the capital would not have shrunk by 1 per cent, and the body of recent immigrants by twice that annual rate: since these urban groups are both included in the grand total, it becomes almost impossible to test his assumptions against specific calculations. It is more precise to say that in the absence of migration to Rome, the non-metropolitan citizen population would have grown by 3 per 1,000 per annum.

as the metropolis expanded. Nonetheless, even in the final year of the period under review, 28 BCE, the supposed non-metropolitan population of 3,350,000 would have increased by another 10,050 persons. In Rome, rates of decrease of 1 per cent for the 550,000 members of the 'core' population (5,500 persons) and of 2 per cent among 100,000 recent immigrants (2,000) together with 0.7 per cent net growth (4,550) created demand for 12,050 immigrants, 2,000 in excess of the available surplus. This might give the impression that as it approached its peak, metropolitan demand had begun to outstrip rural growth. However, this would only apply if the non-metropolitan free population had indeed dropped to 3,350,000. In fact, given that for most of the preceding two centuries, rural growth had considerably exceeded urban demand, the pull of Rome could not completely have absorbed non-metropolitan growth even in the late first century BCE. Instead, the number of free people outside the capital would have risen alongside the urban population: one can calculate, in fact, that, assuming a natural decrease of one per cent of the population of Rome, a natural increase of 0.3 per cent of the non-metropolitan population of Italy, and an immigration of Italian population to Rome such as to counterbalance the natural decrease there and to allow an annual net increase of the population of Rome of 0.7 per cent, the total citizen population of Italy, in 28, would have been 7,015,000, and therefore the non-metropolitan population would have numbered 6,365,000, and not 3,350,000.[15]

The same is true if we assume an arithmetic increase in the urban population at a steady annual rate of 2,500. In this case, in 225 BCE, 4,000 migrants would have been drawn from a surplus of 13,050. This share of thirty per cent is higher than before; conversely, required immigration in 28 BCE would have been lower than in the first scenario, only 10,000 persons. Here, even the (underestimate) of 10,050 non-metropolitan growth would have filled the gap. Then again, rural growth was more substantial because the total population was larger, too. The same qualifications apply as before; again, both the metropolitan and the non-metropolitan populations

[15] The total free population of Italy in each of the years between 225 and 28 can be calculated according to this formula: $Pop\ tot_t = Pop\ non\text{-}met_{t-1}(1+m) + Pop\ Rom_{t-1}(1-in)$, where *Pop tot* is the total free population of Italy, *Pop non-met* is the non-metropolitan population of Italy, *Pop Rom* is the population of Rome, *m* is the 0.3 per cent yearly natural increase of the non-metropolitan population of Italy and *-in* is the 1 per cent yearly natural decrease of the population of Rome.

would have grown during this period. Reality must have been more fuzzy than these schematic models.[16] For this reason, it would make little sense to construct an alternative model with the pretence of offering precise figures. All that matters here is that in any scenario based on Morley's figures, the non-metropolitan population of Italy not only failed to shrink, but managed to grow. The same holds *a fortiori* for the total free population of Italy, which would have increased by the sum of net growth in Rome (500,000 according to Morley) and excess net growth (initially very considerable, gradually decreasing, but never entirely disappearing) outside Rome.

Another serious problem is the way in which Morley arrives at an estimate of the quantitative impact of immigration to Rome on the total population of Italy. It is correct that 7,000 young adult migrants (with a mean age of twenty) were the survivors of an original cohort of about 15,000 newborns. This observation, however, likewise applies to the situation in the city of Rome. Thus, an annual transfer of 7,000 young adults would have been equivalent to a transfer of 15,000 babies. There is no need to assume that any gross population decrease in the capital had to be balanced by the introduction of an identical number of adults.[17] Had, say, 3,000 young adults entered Rome in order to offset the loss of 3,000 inhabitants (1 per cent of 300,000 in the second half of the second century BCE), this event would very likely have resulted in considerable net population growth.[18] In this case, it is useful to conceptualise individuals in terms of their reproductive capacity. Since the fertility of sexually immature persons is nil, fecund adults, albeit only a minority of the total population, account for all reproduction: in this respect, too, 15,000 infants are equivalent to 7,000 young adults. Thus, with balanced sex ratios, far fewer than 3,000 twenty-year olds were

[16] Note that in both scenarios, the mean annual number of migrants is close to 7,000, that is, the average chosen by Morley. This does not mean, however, that this number of *young adults* was required: see below.

[17] This procedure has certainly been suggested by the way in which Wrigley (1967) has calculated how many English young adults in a given cohort had experience of life in London; however, the way this method is applied by Jongman (1990) and Morley (1996) does not appear acceptable, for the reasons given below.

[18] The average person aged twenty was considerably more fertile than a person that represented the average of the total population: the mean reproductive success of infants was reduced by high mortality prior to sexual maturity, and was lower still for the elderly; *per contra*, a young adult faced relatively low mortality during her sexually active period and thus reproductive success which, on average, was about twice that of the average newborn (see below).

required to cover, within a generation, a deficit of 3,000; instead, a bit over half that number must have been sufficient.

This is admittedly a somewhat crude calculation. For instance, if the majority of migrants were male, their mean contribution to reproduction was lower than average: only women can have children. We would be ill-advised, however, to exaggerate the extent of this bias. Without significant female immigration, Rome could never have developed into a mega-city in the first place.[19] Furthermore, model life tables suggest that even naturally contracting high-mortality populations comprised large numbers of children: in the most disadvantaged stable population constructed by Coale and Demeny (Model West Males Level 1), an annual rate of natural decrease of one per cent meant that some forty per cent of the population were younger than twenty.[20]

All in all, we must conclude that Morley's model, in its present form, fails to demonstrate that Rome's demographic pull would have wiped out all population growth outside the capital, let alone caused the rural population to contract. If we take Morley's figure of 7,000 to represent the annual number of migrants, an increase from 4.3 million to 9.4 million would require a rate of natural increase somewhat higher than 4 per 1,000 (and not over 6 per 1,000 as affirmed by Morley): a perfectly reasonable rate of increase in a pretransitional population. And a slight decline from 4.5 million to four million would imply a rate of natural increase among the rural free population of well under 3 per 1,000 p.a., in the order of 1 per

[19] It would be impossible to pretend that all or almost all migrants were young adult males, for under these circumstances, there would have been virtually no reproduction in Rome at all, and the size of the population could never have exceeded rather modest levels. It is worth noticing, by the way, that the population of Rome in the seventeenth century experienced a modest rate of natural increase, notwithstanding a heavy imbalance in the sex-ratio in favour of men: Lo Cascio (forthcoming c).

[20] Coale and Demeny (1983) 105. Even with a 2 per cent decrease, this proportion appears to be of the order of one-third. I hasten to add that an overall long-term annual rate of natural decrease of 2 per cent for the total citizenry of the capital seems highly unlikely, given the implied rates of shrinkage: in the absence of replenishment, the population would have dropped by one-third within 20 years, by two-thirds within 54, and to one-sixth within a century. Hence, Morley's rate of 1 per cent for the more stable 'core' population seems more reasonable. In fact, it does not really matter whether there was any natural decrease at all: at that level, children and teenagers would have made up 40 and 47 per cent of the total at one per cent annual decrease and in a stationary population, respectively; the difference is not particularly striking.

1,000.[21] For what it is worth, it is interesting to see that between 1550 and 1800, the population of London increased almost eight-fold, from 120,000 to 950,000, while the population of England as a whole almost tripled, from 3,010,000 to 8,660,000 (that is, the population of the rest of England apart from London increased 2.66-fold, from 2,890,000 to 7,710,000).[22] Put differently, if we take the guesstimates of the free population of Italy and Rome in 225 accepted by Morley, of 4,500,000 and 150,000, respectively, and assume that the free population at Rome decreased at the very high rate of one per cent p.a., in order to counterbalance this decrease and to facilitate a net increase of more than 0.7 per cent (required to permit growth from 150,000 to 650,000 in two centuries), the rate of natural increase of the rural population of Italy must have been about 1 per thousand. Even if we accepted that Rome was consuming bodies at this very high pace, therefore, there would not be enough reason for believing that migration to Rome alone could have turned a modest natural increase of the free population of Italy into its opposite.

In order to decide which of the two alternative explanations of the Augustan census figures is the most acceptable, we have to check the plausibility of the individual conclusions to which each of them leads, and the consistency of the whole scenario. But one observation must be made in advance. I think it absolutely uncontroversial that there is not a shred of positive evidence in favour of the idea that women and children could be counted in the census; the idea

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[21] Again, it is possible to calculate that, assuming a natural decrease of 1 per cent of the population of Rome, a swelling of this same population from 150,000 to 650,000, and a slight decline from 4.5 million to 4 million of the whole population of Italy from 225 to 28, the consequent decline from 4,350,000 to 3,350,000 of the non-metropolitan population of Italy would have required that the rate of natural increase of the non-metropolitan population of Italy would have increased from −0.07 per cent to 0.2 per cent, according to the following formula: $1 + m = 1 + rt + (i + in)\, q_{rom\ t-1}$, where rt is the rate of yearly natural increase of the whole population of Italy, m is the rate of the natural yearly increase of the non-metropolitan population of Italy, in is the rate of the natural decrease of the population of Rome, q_{rom} is the proportion of the population of Rome in the whole population of Italy.

[22] According to the estimates collected by Finlay and Shearer (1986) 39 table 1. It is worth noticing that it was just in the period from 1650 to 1700, a time of severe epidemics, that the increase from 375,000 to 490,000 of the population of London would have corresponded to a decrease in the population of the rest of England (from 4,855,000 to 4,570,000, that is, much more than the increase of the population of London itself).

itself is implausible, in view of the continued association, which is actually stressed in the account of the *Res Gestae*, of the census with the *lustrum*. Moreover, as I have noticed elsewhere, one can invoke positive evidence for the identification of *civium capita* as adult males in the Greek version of Eusebius' *Chronicon* preserved in Georgius Syncellus, which expressly refers the figure of 14 CE to the *andres*; the word *andres* is used also by the Suda, with reference to the same census.[23] Therefore, it seems to me that the onus of proof falls squarely on those who maintain that, in 28 BCE, even women and children (including or excluding infants) were counted, given that the only argument in favour of Beloch's solution is its alleged demographic plausibility. It follows that in order to undermine the whole thesis, it would be sufficient to show why this alleged demographic plausibility appears to be a chimera.[24] Strictly speaking, in order to refute it, it would not even be necessary to test the demographic plausibility of the alternative explanation.[25] But I can understand that, since Beloch's solution has been so widely accepted by ancient historians, whoever wants to challenge it is obliged to test the plausibility of the implications of either explanation. I embarked on this exercise elsewhere with regard to some aspects of the competing scenarios: for instance, the rural/urban split; the free/slave split; the population density; and the demographic trend from the third century to the first, starting from a careful evaluation of the chapters in Polybius on the general mobilization of the Romans and their allied forces in 225 BCE and of the numerical data that they imply.[26]

The objections that have been raised so far to my revival of Frank's thesis do not seem to me to be cogent. Scheidel does not discuss my reason for believing that the Augustan censuses must have been much more effective, since a new decentralised procedure was adopted, but accepts Brunt's argument that 'overall under-registration may well have been higher than before', 'if women and children had been included for the first time in 28.C.'[27] I must confess I do not see how this could be so, provided that the census would in any case have been based on the *professiones* of the *sui iuris*,[28] and it is

[23] Lo Cascio (1994a, b).
[24] As I have tried to show in Lo Cascio (1994a).
[25] As Morley (1996) 48 n. 87, maintains against me.
[26] Lo Cascio (1994a, b, 2000b); see also (1991/4, 1996 and forthcoming a).
[27] Scheidel (1996a) 167–8; see also Scheidel (1997c) 158 n. 16.
[28] See the clear testimony of the *Tabula Heracleensis*, *FIRA* I² 13 = *Roman Statutes*,

hard to understand why the individual *sui iuris* would have declared a lower number of members of his family than the real one.[29] In order to put underregistration under Augustus at a much higher level than during the Late Republic, one has to assume that the objective of the census itself had changed and that a careful counting was no longer considered important. But again, the only basis for such an assumption would be the Augustan figures interpreted as inclusive of the whole population: again, a circular argument.

As to the census of 28, I do not see why we must suppose that in time of peace men under arms continued to go uncounted, when they had sometimes been included even under the Republic, especially if the decentralised procedure of census was by then working.[30] That means that, accepting Frank's alternative, the number of adult males in 28 must be posited at a lower level than that assumed by Scheidel, and the whole citizen population at a much lower level (at least one million less; I have estimated elsewhere a gross population of about 13,500,000).[31] Moreover, the proportion of the citizen population calculated by Brunt as living in the provinces is probably too low (since the implied number of adult males is too low). Likewise, the share of the citizen population estimated as living in the Po Valley is certainly much too small.[32] I do not understand how we may posit such a low level of population density in Northern Italy, the richest region, in agricultural terms, of Roman Italy, as Polybius already knew very well,[33] if the percentage of the population of Northern as opposed to Peninsular Italy on any count was so much higher in early modern Italy (as I have already noticed against Jongman).[34] In my view, the much smaller number of towns in the North is no argument, if these towns were bigger on average than Peninsular towns[35] (for example Brixia, Patavium, Mediolanum,

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24, ll. 142–58, on which Brunt (1987) 15–16; Crawford and Nicolet, in Crawford (1996) I 389–90; and Lo Cascio (1990, 1997).

[29] If ever, the opposite would have applied at least to the census of 14 CE as a consequence of the Augustan marriage legislation, which rewarded fecundity: see references, e.g., in Raditsa (1980) and Mette-Dittmann (1991).

[30] On this decentralised procedure see Lo Cascio (1997, forthcoming a).

[31] Lo Cascio (1994b).

[32] Brunt (1987) ch. 13, with my comments in Lo Cascio (1994b); see now Bandelli (1999).

[33] Pol. 2.15; Polybius himself notices in this same context, and referring to his times, *to . . . plethos ton andron* (2.15.7).

[34] Above, n. 6.

[35] As the epigraphic density would suggest: see the calculations by Duncan-Jones

Ticinum, and others). And in any case, the number of towns, whatever their size, cannot be held to be a proxy for the whole population. To suppose that Northern Italy was in fact much more populous than Beloch and Brunt would allow has two important consequences: it helps to explain why the census figure for 70–69 is so low (even if I do not see it as intrinsically improbable that the census of 70–69, if centralised at Rome,[36] counted just thirty per cent of the male adult citizens); and it makes a much more reasonable and correct basis for comparison between the population of Roman Italy and the population of early modern Italy.

Finally I do not see the reasons why we must put the proportion of mainland Italy settled in Roman times at about forty per cent, when we know from the surveys that in some regions of Italy in the Roman period marginal lands were brought into cultivation to an extent that was never to be achieved afterwards[37] and when we know that the area of settlement was fifty-five per cent in 1881.[38]

I do not want to repeat what I have written elsewhere about the difficulty of matching the number and the probable average size of the 430 towns of Italy with the supposition that no more than four million people lived on the land in Italy.[39] Scheidel forgets that even if grain for Rome came from the provinces, to satisfy all its other alimentary needs (from wine to vegetables and meat), Rome depended on Italy and chiefly on its central regions, and therefore heavily affected agricultural production, as Morley's book now shows. Hence, Scheidel's comparison with Italy in the sixteenth century, boosting the population of Naples from 212,000 to 800,000, the size of late Republican Rome, simply does not work. He neglects as well that the average size implied by Hopkins' guesstimate of the population of the Italian towns is impossibly low, as the epigraphic evidence studied by Duncan Jones can show.[40]

It seems to me that a further strong argument in favour of Frank's explanation comes precisely from what Scheidel has observed about

(1982) 339 table 9 (234 and 180 inscriptions per town in Regions X and XI as compared to 33 and 23 in Regions II and III).

[36] As Cic. 1 *Verr.* 54 seems to imply: Wiseman (1969) 68–9.

[37] See the literature quoted in Lo Cascio (1994b); and some of the papers in Lo Cascio and Storchi, eds. (forthcoming).

[38] Hopkins (1978) 7 n. 13; this estimate of forty per cent is the one given by Brunt (1987) 126 and Hopkins, following Nissen.

[39] Lo Cascio (1994a, b).

[40] Duncan-Jones (1982) ch. 6.

recruitment in the first period of the Augustan rule. Starting from the size of the Augustan army of twenty-eight legions, thirty-five cohorts of Roman citizens and twelve metropolitan cohorts, and from his acceptance of Beloch's explanation, Scheidel observes that it would have been necessary to recruit about twenty-two per cent of the 49,200 men who would have turned twenty every year 'in order to ensure the annual influx of some 11,000 recruits required to keep these units at least at 90 percent nominal strength' (the simplifying assumption being that the mean age of enlistment was precisely twenty).[41] Based on Brunt's and Hopkins' figures for the number of men under arms and on the census figures, Scheidel observes also that 'if... we posit a mean duration of military service of 8 years during the period from 193 to 133 BC, on average almost 50 percent of all men aged 17 would have had to enlist. About 12 percent of all male adult citizens fit for military service would have been soldiers at any one moment. In this period, about 7.5 percent of all man-years lived by all males from cradle to grave would have been spent in the army'.[42] I think that these observations raise a general point: even if we accept the militarism of Rome as something which affected decisively its political, demographic, economic and social development, does our evidence really oblige us to accept that the burden of recruitment was such as to be absolutely incomparable with what is known of other preindustrial states, for which we have better evidence, including even comparably aggressive and expansionistic states? I have summed up the data that emerge from the analyses of Brunt in Table 1.[43] As for the census figures, I have not corrected them in order to account for under-registration (put at ten per cent by Brunt):[44] it seems to me completely unwarranted to posit a steady level of underregistration (which could be manipulated by us to fit any general reconstruction); moreover, my essential aim is to show that we need to suppose a very strong level of underregistration in order to accept the general picture given by the census

[41] Scheidel (1996a) 93.
[42] Ibid. 94.
[43] Note that the general conclusion drawn by Brunt has been widely accepted and that it provides the basis for the analysis of Hopkins and of his further calculations of the military effort of Republican Rome: see Hopkins (1978) 25 ff. with table 1.1 and figure 1.3; see also Nicolet (1976) 150 ff. and (1977) 310–12; Harris (1979) esp. 44 ff.; and now also Patterson (1993).
[44] Brunt (1987) 35 and passim.

figures. In addition, like Brunt, I have taken men in arms overseas as always uncounted,[45] and I have posited the casualties for the worst periods of warfare at the levels estimated by Brunt,[46] and not at the levels suggested by our sources. One of the main foundations of Brunt's argument is in fact precisely his assumption that the casualty figures given in our sources are always greatly inflated: as Brunt himself has observed, 'if the highest figures for Roman losses in 218–216 are accepted, the conclusions on the proportion of *iuniores* under arms and on the ratio of *assidui* to all adult males . . . must be modified. . . . The figures for the 80s, if admissible, would cast grave doubt on my estimate of the Italian population at the time'.[47]

Even by adopting the figures Brunt proposed, what emerges is a ratio of men under arms: *iuniores* or men under arms: whole citizen population which, taken as a whole, is without parallel: the ratio men under arms: *iuniores* is never less than 1:5.46, and during the Hannibalic war and the civil wars it attains levels of much more than 1:2 (which means that much more than a half of the males between seventeen and forty-six were under arms); the ratio of men under arms: whole citizen population is never less than 1:25 and it attains levels as high as 1:7 (or even 1:6) or 1:8. In my Table 2, I have reported data referring to several countries in modern Europe, taken from Corvisier's *Armées et sociétés en Europe de 1494 à 1789* (1976) and from Tilly's *Coercion, capital, and European estates* (1992). Other data can be found in Parker's *The military revolution* (1996), and McNeill's *The pursuit of power* (1982). I can add, for the sake of completeness, a reference to Ch'in China, which had a permanent army of 1,000,000 in a population of 50,000,000 (that is, two per cent, or six per cent of all adult males) or, more important, the case of Napoleonic Francc after the *levée en masse*: it has been calculated that all over the French Empire from 1800 to 1814, thirty-six per cent of the men who could serve were conscripted, that is, seven per cent of the total population.[48] The assembled data refer to very disparate

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[45] Unless stated otherwise in the sources: see Liv. 29.37.5 on the census of 203; Brunt (1987) 63 n. 2, 68.

[46] Brunt (1987) 694–7 and passim.

[47] Brunt (1987) 696.

[48] Parker (1996) 3; Mounier (1987) 460; see also Frasca (1993) 142, for Napoleonic France; as a comparison, Frasca quotes the data referring to the First World War: between 1914 and 1919, 8 million were drafted, that is, 20 per cent of the whole population of France at the beginning of the twentieth century.

situations: from the cases of professional or mercenary armies to the first attempts at conscription, through systems, like the *indelningsverk* in seventeenth century Sweden,[49] which remind us of the criteria adopted in the Later Roman Empire with the *capitula* or *consortia* of landowners and the *praebitio tironum*, recruits selected among the rural population of *coloni*.[50] It is only in very peculiar and short-term situations that European countries attained levels for the ratio of men under arms/total population comparable to the average level assumed by Brunt's and Hopkins' calculations for the last two centuries of the Republic. The levels imputed for the Hannibalic war, the social war and the civil wars are simply without parallel.

True, it must be underlined that for somewhat more than half of the period under scrutiny there was just conscription in Rome and there were as a rule no volunteers: but conscription affected only *assidui* and therefore a changing proportion of *iuniores*, whose size is impossible to estimate at any given time (with the exclusion, as will be shown in a moment, of a specific case). It is symptomatic, however, that Dionysius says that already at the time of King Servius Tullius *proletarii* outnumbered *assidui*, whereas Cicero claims that in Servian times the single century of *proletarii* comprised more men than almost the whole of the first class.[51] It is almost certain that these statements reflect the state of affairs in their own time (or, better, in the time of their sources).[52] But we cannot rule out Dionysius' statement as a mere impressionistic evaluation and therefore we have to take into account that the military burden affected only part of the adult males and that the ratio between men under arms and *iuniores assidui* must at any time have been much higher. After Marius'

[49] Corvisier (1976) 62 ff., 132 ff.; Parker (1996) 52 ff.

[50] See especially Mazzarino (1951) ch. 6; Jones (1964) ch. 17; and see now Carrié (1993) 137 ff. and Lee (1998).

[51] Dion. Hal. 4.18.2; cf. 7.59.6; Cic. *De Rep.* 2.40.

[52] Brunt (1987) 24; according to the most commonly held view the process of proletarianization of the army was well under way when Marius began to appeal to the *capite censi* for volunteers, as a result of the reduction of the minimum census of the fifth class, which would have been posited at 1,500 asses before 129, according to the testimony of Cic. *De Rep.* 2.40; but I do not believe that the figure in Cicero refers to the dramatic date of the *De Republica*: as I tried to show elsewhere, the figure (actually 1,100 asses) represents the minimum qualification (in terms of libral asses) before the reduction of the weight standard of the as: Lo Cascio (1988). We must confess that we do not know what the minimum qualification of the fifth class was at the end of the second century and we can only speculate, therefore, on the respective number of *assidui* and *proletarii* at that time.

reform, when recruitment of volunteers became important, it is even harder to justify such proportions of men under arms. It is harder especially if we do not assume, with Brunt, that after Marius *proletarii* were recruited not only as volunteers, but they were obliged to enlist: a conclusion which appears to me to be unwarranted.[53]

For these reasons, the scenario built by Brunt seems to me implausible, and the only way to avoid it is to think: 1) that the Augustan figures refer to adult males; 2) that the Republican figures, drawn from a registration for which personal attendance of the *sui iuris* at Rome was necessary, must have been extremely defective. In particular those who had no obligation to serve, the *proletarii*, must have gone underregistered and then undercounted: for them the obligation to present themselves in front of the *censor* to give their *professiones* must have been lightly enforced, if enforced at all, since for them, there had never been an individual *dilectus*. They were levied just in emergencies through the *tumultus*, the *levée en masse*.[54]

In order to strengthen my case I shall comment in more detail on some of the most crucial periods: the period of the Hannibalic

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[53] Brunt (1987) ch. 22, esp. 408; see the opposite and in my view correct conclusion of Smith (1958) 44 ff., on the basis of the clear testimony of Sallust, *Iug.* 86.2; this conclusion seems implied in Gabba (1973) 35 ff., 47 ff., in Harmand (1967) 14 ff., 245 ff. and (1969), whereas I cannot say whether it is also held by Nicolet (1976) 173 ff.; see also, e.g., Keppie (1984) 61, who distinguishes between the juridical situation and the *de facto* situation. Again there is a strong element of circularity in Brunt's conclusion, since his main argument (or, I would say, his unique argument) for thinking that after Marius *proletarii* might be obliged to enlist is precisely that otherwise the proportion of men serving in the legions on the total male population as estimated by him would become hardly credible . . . (see especially what Brunt observes at 410: 'But if Appian was right in holding that most of the veterans in 41–40 were proletarians, or if the generally accepted view is correct that the proletarian element predominated in every army within this period, it would seem to follow from the premiss that only assidui were conscribed that there was a vast reservoir of poor citizens ready to enlist, and that in civil wars 100,000 or more volunteers could be found in this class', a conclusion that Brunt could not arrive at, given his estimate of the whole male population). Of course, *proletarii* could be made to serve in case of *tumultus*, as was the case even before Marius' reform. Nor has one to conclude that with the new system of the *conquisitores* things would have changed.

[54] Gabba (1973) 20–1 and *passim*; Brunt (1987) 629–30; Nicolet (1976) 129–30 on *tumultus*; that is the reason why we do not have any evidence of the enforcement of the very severe penalties for the *incensi*, as Brunt himself notes ((1987) 33 ff.), whereas we do have evidence of the enforcement of the penalties for the citizens who did not respond to the *dilectus*, and quite understandably these penalties were the same as the penalties for the *incensi*: on this whole issue see Lo Cascio (forthcoming a).

war, the seventies of the first century BCE, and the years of the Second Triumvirate. The proportion of young males in service during these periods is dramatically high, regardless of different hypotheses concerning the length of service.

First, the Hannibalic war. Brunt starts from a passage in Livy according to which in 214 it was discovered that only 2,000 *iuniores* had evaded military service during the preceding four years of war. Brunt maintained that, if there were just 2,000 people who had never served in 214, the total of *iuniores assidui* could not have been more than, say, 100,000–120,000. Now Livy says that 2,000 were the men who could not plead illness or 'vacatio iusta militiae'.[55] Even if we do not accept the suggestion by Rich that 'the 2,000 men singled out for punishment in 214 were by no means the only qualified men who had not served since 218 . . . but those who had not presented themselves at the levy',[56] it is incredible that there would have been a total mobilization of the *assidui*, from seventeen to forty-six years. In his total, Brunt has not taken into account the men who had already served for several years before 218, those men, I would say, that could plead a 'vacatio iusta militiae'. Now, if we reckon with the levy of four legions of 4,500 men each every year before the beginning of the second Punic War and retain the number of men under arms for the years from 218 to 214 as calculated by Brunt, and if we assume that the length of service was sixteen years,[57] the number of the men who could plead a 'vacatio iusta militiae' in 214, that is, of the survivors of the age-classes between thirty-three and forty-six who had already served for sixteen years, must have been considerable;[58] it would be somewhat lower, of course, if one took the maximum length of service posited by Polybius at twenty years[59] (and therefore took into account the survivors of the age-classes between 37 and 46). However, it would drop to zero only if we made the assumption that the maximum length of service was thirty

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[55] Liv. 24.18.7–8; on the *causarii* see Brunt (1987) 629.

[56] Rich (1983) 294.

[57] According to the datum, by no means certain, normally drawn by a corrupt passage in Polybius 6.19.2, on which see in particular Walbank (1957) 698; Brunt (1987) 399; possible alternatives would be six or ten (on the basis of Plut., *C. Gracch.* 2.5).

[58] Even if it is impossible to estimate it: we can of course estimate the whole number of survivors of these age classes, but not how many of them would have served for sixteen years.

[59] Pol. 6.19.2.

years (that is, from seventeen to forty-six). To this number we have to add the number of people unfit for military service, the number of Campanians who defected to Hannibal (40,000 adult males according to Brunt,[60] but it can be shown that this is a very low estimate[61]), and of course the number of the dead. Brunt put military fatalities from 218 to 214 at 50,000, even though the casualty figures given by Polybius suggest a much higher total of 110,000.[62] Finally we have to consider the number of the men who were actually serving in 214, more or less 70,000 (excluding the *volones*).[63] If we put the grand total of the discharged, people unfit for military service, Campanians, dead and still serving against the number of *iuniores* which is estimated by Brunt for the beginning of the Second Punic War (and even accepting the idea that the number of adult males would have risen somewhat since 225, from 273,000 to 300,000), we reach an astonishing result: that many more than fifty per cent of *iuniores* still alive in 214 were already under arms and that a substantial share of the people not serving could plead a 'vacatio iusta militiae'. Most of those not serving must have been *proletarii*; hence, it is quite conceivable that it was precisely in 214 (in my view by exploiting the reduction of the weight standard of the *as*)[64] that a reduction in the property qualification of the fifth class allowed the recruitment of the least poor among the *proletarii*, now turned into *assidui*.

Is the ratio of men under arms/*iuniores* implied by these figures at all plausible? I think not. And it deserves notice that even Brunt is conscious of the difficulties at this point, since he asks: 'Is it not a mere absurdity to suppose that after losing 50,000 men, Rome could still provide 75,000 for her armies and fleets in 215 and increase her manpower effort in subsequent years?'[65] I agree that it is, in fact,

[60] Brunt (1987) 64 on the evidence of Liv. 23.5.15.
[61] Savino (1997).
[62] Brunt (1987) 419, cf. 65.
[63] Brunt (1987) 418 table X and passim.
[64] Presumably the quadrantal reduction: see Lo Cascio (1980/81, 1988, 1999d).
[65] Brunt (1987) 66, on the basis of his calculations at 417 ff. It is interesting to observe that Beloch himself in the *Italische Bund*, before changing his mind about the meaning of the Augustan, and therefore also of the Republican, census figures, thought that what we know about the military effort of Rome during the Hannibalic war ruled out the possibility that the Republican census figures included the *seniores*: 'Wir sehen, Roms militärische Leistungen im hannibalischen Kriege bleiben absolut unverständlich, wenn wir die Annahme fest halten, die Censuszahlen hätten auch die seniores umfasst' (Beloch (1880) 242).

a mere absurdity, if one puts, with Brunt, the number of deaths on duty (excluding the *volones*) between 214 and 203 at 75,000.[66] It must be pointed out that on any estimate the share of slaves in the total population cannot have been as high as in the second or first centuries. Thus, one has to ask who could have produced enough food for the whole Roman population if perhaps more than half of the surviving adult males were at war. But the alternative to accepting this absurdity is not to 'discard the annalistic evidence on which such figures are based', which is the alternative favoured by Brunt, but to discard the basis of his calculation, the number of adult males as revealed, according to him, by the Polybian figures for 225 BCE, of 273,000,[67] a figure Brunt raises to 325,000, assuming ten per cent underregistration and an alleged high rate of natural increase in the years between the two Punic wars.

We may assume that the number of *iuniores* in 218 was much higher than that advocated by Brunt, because we must understand the famous Polybian passage on the events of 225 differently from Beloch and Brunt, who have assumed that Polybius had made a gross mistake.[68] I cannot dwell extensively on this point here, only very briefly sum up what I have written elsewhere.[69] Introducing his narrative of the Gallic invasion in 225, Polybius states the numbers of the various forces on which Rome can rely, listing them according to an order that is perfectly logical in the light of the Roman strategy of defence. First of all, Polybius reports the numbers of the Romans and the allies under arms. Following this list of men under arms and ready for combat, Polybius mentions the reserve at Rome and gives its numerical strength, then he refers to the *katagraphai* (the documents giving the number of men of military age) sent by the communities of Central and Southern Italy, with the exclusion of the Bruttians and the Greeks and with the exclusion, obviously, of the communities in which the emergency levy had already been conducted. Then he indicates the strength of the two legions stationed in Sicily and at Tarentum, and the total number of Romans and Campanians on the rolls (250,000 foot and 23,000 horse), a number which ought to correspond, in its meaning, to the one declared by the allied communities in their *katagraphai*. Finally, Polybius gives

[66] Brunt (1987) 714.
[67] Pol. 2.24–5.
[68] Beloch (1886) 355 ff.; Brunt (1987) ch. 4.
[69] Lo Cascio (2000b) and (forthcoming a) ch. 3.

the total number of the men 'able to bear arms' (more than 700,000 foot and more or less 70,000 horse). It has been easy to see in this total the grand total of the forces enumerated by Polybius: a grand total which is built, as required by the logic of the situation and as expressly stated by Polybius, by summing up *all* the various items mentioned.

The Polybian passage seems absolutely clear and his total perfectly consistent with the presentation of the Roman strategy given by Polybius himself, whose source is here Fabius Pictor, a contemporary of the events; his listing of the forces presupposes, in fact, the various stages of this strategy. The obvious conclusion is that, according to Fabius-Polybius, the total of the Roman and allied forces is correctly made up of the total of men under arms plus the total of the forces mentioned in the *katagraphai*; these *katagraphai*, like the figure of Romans and Campanians which appears at the end of the list, do not refer to the total of the men on the registers before the normal levy and the emergency levy had taken place, that is, including the men under arms; instead, they refer to the remaining total of the men on the registers once the normal levy and the emergency levy had already taken place: that is, when the various armies mentioned by Polybius had already been deployed.

This conclusion, which seems to me absolutely necessary, if we trust Polybius, was the conclusion of Mommsen.[70] Subsequently, however, it was subjected to radical criticism by Beloch and most of the scholars who have since dealt with this problem.[71] According to Beloch, Fabius made a crass error: by adding up all the forces that Rome could oppose to the invaders, he calculated the Romans and the allies under arms twice: the actual total of the Romans, including the men already under arms, is the figure that Polybius gives at the very end of the list, as the figure of the Romans and Campanians: 250,000 foot and 23,000 horse; and the single totals for every group of the allied communities, including the men already under arms, are the ones indicated in the *katagraphai*, a figure representing the total number of *iuniores* available in these communities. What Fabius had at his disposal were just the *katagraphai*, from which he would

[70] Mommsen (1876).

[71] Beloch (1886) 355 ff.; Afzelius (1942) 98 ff.; Walbank (1957) 196 ff.; Toynbee (1965) I 479 ff.; Brunt (1987) ch. 4; and now Baronowsky (1993). Marchetti (1978) 141 ff. and Shochat (1980) 32 ff. follow Mommsen in assuming the total of Romans and Campanians given by Polybius to exclude men under arms.

have derived his figures: it would have been impossible for him to know the numbers of the men of every group of communities under arms, in order to subtract them from the *katagraphai*.

The fundamental reason why Beloch thought that the Polybian figures had to be understood in this way is that only by understanding them in this way it is possible to consider the figure of the *cives Romani* given by Polybius to be in line with the census figure for 234/3 (or for 239/8)[72] known from the Livian *periocha* and interpreted as representing all the adult males. Once one supposes that Fabius must have taken his total for Romans and Campanians from the last census figure (that is, from the unknown figure for 230/29), one has to conclude that the figure given by Fabius cannot be much higher than the figure of the *periocha* for 234/3 (or for 239/8). This figure is 270,212 *civium capita*. If we assume that there were 325,000 *civium capita* (that is, the Romans under arms plus the Romans and Campanians on the registers), the increase over the figure for 234/3 (or for 239/8) would be hardly credible as the result of a natural increase of population; therefore, it seemed more economical to think that a gross mistake was made by Fabius in calculating twice the number of Romans under arms. If the total of *civium capita* in 225 was just 273,000, the increase would be very reasonable: a mere three thousand.

The flaw in Beloch's theory is to suppose that the apparent similarity between the figure of the *periocha* and the figure given by Polybius for the Romans and Campanians would show that the figures were *in pari materia*. But nothing could be more debatable. The census figures during the third century comprised all the registered adult males in the tribes, including the *seniores*, and therefore probably excluded the *cives sine suffragio* (who were not registered in the tribes).[73] Moreover, the proportion of *proletarii* who registered themselves must have been low: the very fact that the penalties for failing to register were the same as the penalties for those who failed to present themselves at the levy seems to suggest that the primary purpose of the obligation to register was to prevent evasion from military service, and since *proletarii* were not individually called to serve in the army and were recruited in very rare emergencies *en masse*, there would have been no point in insisting on their registration.[74]

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[72] Marchetti (1978) 146–7, and n. 22, on the evidence of Liv., *Per.* 20.
[73] Lo Cascio (forthcoming a) ch. 1, and (forthcoming b).
[74] See above, n. 54.

The expressions that Polybius used and that were derived from Fabius himself seem to imply that the figures he gives are representative of the total mobilization of the *iuniores*.[75] The *katagraphai* required of the *socii* were to exhibit the number *ton en tais elikiais*, that is, of 'those in the ages', the men of military age. This expression obviously translates the Latin *iuniores*. Dionysius of Halicarnassus uses the expression *oi echontes ten strateusimon elikian* ('those who have the military age') for conveying the same concept (as opposed to *oi en ebe*, which included also the *seniores*).[76] The total number (*sympan plethos*) of the men that can stay the invaders is the total number *ton dynamenon opla bastazein*, 'of the men able to bear arms'. The expression translates the Latin 'eorum qui arma ferre possent', used by Livy (but also by Caesar, with reference to the Gallic armed forces) evidently to refer to the physical fitness for combat that depended on age.[77] Moreover, the Polybian passage seems to suggest that the emergency levy was conducted regionally (for the first time?). And that is the reason why we get totals for each geographical or ethnical entity, and irrespective of the distinction between citizens and allies. The procedure of the regional levy must have been adopted not only in the conscription and gathering of the allied forces, but of the Romans forces as well; and that is why we have a total of Romans and Campanians, or of Etruscans and Sabines, citizens and allies.

If the Polybian figures are conceived of as maximum figures of men able to bear arms, that is, of *iuniores, assidui* and *proletarii* combined, we arrive at a figure for all adult males which is much higher than the figure revealed by the last census, which apparently did not include the Campanians *cives sine suffragio*, but did include *seniores*, and therefore must have been seriously defective. If we put the number of *iuniores* in 225 at 360,000 (the figure suggested by the Polybian passage) instead of 195,000, and therefore the number of adult males at 500,000 instead of 273,000, Roman manpower capacity during

[75] Lo Cascio (1991/94) 324 ff.

[76] See e.g. 4.15.6; 4.16.1; 5.45.8; 8.17.2 etc.; *oi en ebe* contrasted to *oi echontes ten strateusimon elikian*: e.g. 5.75.4; *oi en ebe* for the results of the census: 5.20.1; 5.75.4; 6.63.4; 9.25.2; 9.36.3: see Lo Cascio (forthcoming a) ch. 2.

[77] Liv. 3.4.10; cfr. 1.44.2; Caes., *BG* 1.29.2; see Lo Cascio (forthcoming a) ch. 2; a different view in Gabba (1973) 19 with n. 54.

the Hannibalic war attested by the figures of the legions in service and of the casualties becomes credible.[78]

This interpretation of the Polybian passage has additional merits. It implies that the census figures referring to the second century must have been seriously defective as well: correspondingly, the burden of conscription is again reduced to more plausible levels. It also allows us to put the rate of population increase in what in 225 was the *ager Romanus* from the third century to the first centuries at a very reasonable level.[79]

A similar argument may be advanced for the figures after the Social War. First of all, Appian (*BC* 1.39) records the number of men raised by the Romans and the rebels in 90: both were able to field 100,000. But the military commitment during the war was even bigger. Brunt himself considers it probable that the figure given by Velleius as the figure of the dead during the war, 300,000, was in fact the figure of the men under arms in 90–89: and he estimates that 175,000 of them would have fought on the Roman side.[80] This figure of men under arms amounts to more than half the total number of *iuniores* suggested by the last census figure before the Social War. The ratio implied by the figures given by Brunt for 87 and 83–1 is even higher.[81] Such estimates of the level of military commitment compel Brunt not only to maintain that the armies of the civil wars were mostly made of conscripts, but even that conscription would also have extended to *proletarii*. However, it seems to me that the only reason for such a conclusion, which is not independently borne out by our evidence, is precisely that only by assuming that there was a conscription of *proletarii* does the alleged ratio men under arms/*iuniores* become plausible: it is obvious that armies composed to a large extent of volunteers usually represent a smaller share of the adult male population than armies of conscripts. Brunt himself is aware of that, since he observes: 'Given the great size of armies in certain decades of the first century, I would be surprised on my estimate of the free population if the evidence showed that

[78] See Lo Cascio (2000b) 169 table 13.1, for an attempt at quantifying the population of Italy in 225 starting from the Polybian figures as interpreted above.

[79] As it appears from the calculation made in relation to the territory of the Romans and their allies in 225: ibid. 170.

[80] Vell. 2.15.2; Brunt (1987) 439.

[81] Brunt (1987) 440 ff.

most soldiers were volunteers, and would regard it as a strong objection to that estimate'.[82]

Finally, there is another point in Brunt's reconstruction which again seems unwarranted: the importance he seeks to assign to the recruitment of *liberti* and even *peregrini* in the so-called 'vernacular legions' after 49 BCE. This assumption allows Brunt both to put the ratio of men under arms/*iuniores* in the 40s and 30s at a more reasonable level, and to play down the number of Roman citizens in the provinces.[83]

This raises a crucial issue: the most serious argument against Frank's explanation of the Augustan census figures is thought to be that it entails an implausibly high estimate of the population of Italy in comparison with the population that country had attained by the fourteenth to seveenth centuries. In the light of the presumed changes of the Italian population between, say, the late second century CE, when the population began to contract as a consequence of the outbreak of the so-called 'Antonine plague',[84] its undoubted collapse after the sixth century[85] and its recovery from the eleventh to the fourteenth century,[86] I do not see any reasons for considering this argument truly compelling. In fact, the lower we put the population of Italy in the seventh or eighth century, the higher the annual rate of increase in the centuries between the seventh or eighth and the fourteenth becomes.

Moreover, the whole argument is of course much weakened if one puts the number of the Roman citizens in the provinces in 28 BCE at a level much higher than the 1,200,000 allowed for by Brunt. If a greater proportion of the 13,500,000 *civium capita* in 28 BCE that can be derived from the attested 4,063,000 adult males actually resided in the provinces, the Italian population would not have been more numerous than in the seventeenth century.[87]

[82] Brunt (1987) 415.
[83] Brunt (1987) 509 ff. and passim.
[84] See Lo Cascio (1991) 707 ff. and now Duncan-Jones (1996).
[85] See now Pinto in Del Panta *et al.* (1996) 17 ff.
[86] Pinto in Del Panta *et al.* (1996) 27 ff.
[87] Lo Cascio (1994b).

Table 1

A. Men under arms (data from Brunt)

	Legionaries	*Iuniores*[1]	Adult males
225		(195,000)	273,000
217 (before Trasimenus)	50,000	(216,000)	(300,000)[2]
216 (before Cannae)	65,000	(191,000)[3]	(275,000)[4]
215 end	60,000[5]	(126,000)[6]	(190,000)[7]
214	75,000[5]		
212	80,000[5]		
(To these *proletarii* and *liberti* must be added, who served in the fleet: Brunt reckons with 20,000 in 218 and 15,000 in 212).			
194/3	44,000	(112,000)	143,704 (155,000)[8]
189/8	66,000	(210,000)	258,318 (291,000)[8]
179/8	44,000	(202,000)	258,794 (281,000)[8]
174/3	38,500	(206,000)	269,015 (285,500)[8]
169	45,600	(249,000)	312,805 (346,000)[8]
168	58,200		
89	150,000		
[90/89: both sides	300,000]		
[90/89: Romans	175,000]		
87	200,000		
86		(355,000)	463,000 (493,000)[8]
83–81	272,000		
71	160–200,000		
70/69		(705,000)	910,000 (980,000)
49	156–172,000[9]		
43	(264–330,000)[10]		
28/27		(828–972,000)	(1,150–1,350)

B. Ratio mobilized: all *iuniores*

217 (before Trasimenus)	1:4.32
216 (before Cannae)	1:2.94
215 end	1:2.1
214	1:1.68
212 (incl. proletarii & liberti)	1:1.32
194/3	1:2.55
189/8	1:3.18
179/8	1:4.6
174/3	1:5.35
169	1:5.46

168	1:4.28
83–81	1:1.3[11] (1:2.6)[12]
71	1:4.4 or 1:3.525
43	1:2.51–1:3.68 (according to the figure for 28)

C. Ratio mobilized: whole citizen population (if adult males account for thirty per cent of the total)

217 (before Trasimenus)	1:20
216 (before Cannae)	1:14
215 end	1:10.55
214	1:8.44
212 (incl. proletarii & liberti)	1:7.91
194/3	1:11.74
189/8	1:14.7
179/8	1:21.3
174/3	1:24.7
169	1:25.3
168	1:19.8
83–81	1:6 (1:12)
71	1:20.4–16.33
43	1:11.6–1:17 (according to the figure for 28)

[1] Seventy-two per cent of all adult males.

[2] According to Brunt (but without considering the alleged under-registration of ten per cent).

[3] Assuming 25,000 *iuniores* dead.

[4] Putting the decrease at 25,000, the same affecting *iuniores*.

[5] Including 8,000 *volones*.

[6] Assuming 25,000 dead and excluding the Campanians, estimated at 40,000 by Brunt.

[7] Putting the decrease for death at 25,000, the same affecting *iuniores*.

[8] Including the men in the legions overseas who went uncounted, according to Brunt 72, table VII.

[9] Plus the Italians enlisted in the provinces by the Pompeians, who are not counted (Brunt 509).

[10] 66 legions, nine to ten of which were 'vernacular': Brunt posits the Italians at 216,000 to 270,000, the provincials at 48,000 to 60,000.

[11] If we adopt the census figure for 86.

[12] If we adopt the census figure for 70.

Table 2: Ratio of men under arms to total population, Europe 1600–1850

Country	Dates	Ratio
Spain	1600	1:40
	1700	1:143
	1759	1:160
	1850	1:100
France	1600	1:250
	1700	1:48
	1710	1:66
	1738	1:165
	1760	1:85
	1789	1:145
	1850	1:83
Austria	1705	1:80
	1786	1:96
Piedmont	1734	1:54
	1738	1:75
Great Britain	1600	1:143
	1698	1:410
	1710	1:150
	1747	1:100
	1783	1:310
	1850	1:91
Netherlands	1600	1:77
	1700	1:19
	1850	1:100
Prussia	1740	1:27
	1760	1:14
	1786	1:29
Sweden	1600	1:67
	1709	1:13
	1850	1:56
Russia	1600	1:333
	1700	1:83
	1796	1:120
	1850	1:67

Sources: Corvisier (1976); Tilly (1992).

CHAPTER FOUR

MORE IS WORSE: SOME OBSERVATIONS ON THE POPULATION OF THE ROMAN EMPIRE

Bruce W. Frier

This paper is largely theoretical, and my aims are narrowly limited.[1] Although my subject is the early Roman empire, I am not attempting to estimate its gross population, nor the populations of its constituent provinces or regions; nor do I express any view on whether these populations were actually growing or shrinking during this period. All these topics are unquestionably of immense historical importance. However, with regard to each of them I am, for the time being, agnostic. The reason is that surviving evidence on population seems to me exceedingly fragile both in its quantity and quality, and accordingly I have little confidence in our ability to arrive at more than vague (if arguably 'educated') guesses as to gross population levels and change.

From the outset of my research on Roman demography a little over two decades ago, my interest has lain in areas where, despite what I concede are still formidable evidentiary obstacles, I have felt that we can nonetheless hope to make progress. These areas involve the major underlying demographic realities of the early Roman empire, particularly as concerns subjects such as mortality, nuptiality, and fertility. Our knowledge of these subjects is, of course, still very imperfect. We have only occasional avenues of opportunity, and each is fraught with uncertainty at many crucial points. Nonetheless, these avenues have at least the virtue of being relatively untrodden; and, as I hope to show, they may also provide some indirect help on questions of gross population.

Let me begin by returning to a section of *The demography of Roman Egypt*, the book that Roger Bagnall and I wrote on what is probably

[1] For this reason, I have preserved the original format of this paper, with only the lightest annotation. Newell (1988) outlines basic demographic concepts and working methods.

our best single source for ancient demography, the early imperial census returns from Egypt.[2] Towards the beginning of this book, we briefly discuss the gross population of Egypt. This matter was not, to be sure, of great moment to our project. For the most part, our discussion drew heavily from Dominic Rathbone's persuasive article concerning the population of Graeco-Roman Egypt.[3] However, there was a significant difference in emphasis between Rathbone's article and our book. Though we did point out this difference, we did not stress it; but it has important implications.

As is well known, the two main literary sources for Roman Egypt's population appear to contradict each other. Towards the end of the first century BCE, Diodorus Siculus (1.31.6–9) places Egyptian population at three million. By contrast, not quite a century later the historian Josephus, in a literary speech (*BJ* 2.385), gives a population of 7.5 million for Egypt exclusive of Alexandria, an estimate that is allegedly based (by what computation is unclear) on the amount collected from the poll tax levied on adult males; this population would imply a total Egyptian population on the order of eight to nine million. It is improbable that both Diodorus and Josephus are correct.

Historians of Roman Egypt, doubtless influenced by a commendable zeal to enhance the luster of their subject, have usually opted for Josephus' higher estimate. Accordingly, they have denigrated or crudely emended away Diodorus' figure. Rathbone, by contrast, supports the Diodorus estimate on a variety of grounds, but particularly because of the likely 'carrying capacity' of the Nile valley before the late nineteenth century, prior to the introduction of perennial irrigation and Egypt's partial integration into European industrial economies.

Bagnall and I used Rathbone's argument as our starting point, albeit with several revisions especially in the number and size of the populations of the district capitals, the *metropoleis*. However, the drift of our argument was neither to support Diodorus nor to undermine Josephus. To put it bluntly, stray population figures from ancient literary texts struck us as unimpressive in the absence of clear and convincing evidence as to their origins and interpretation; and cer-

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[2] Bagnall and Frier (1994) 53–7. I should stress that Roger Bagnall is not implicated in the argument I am making here.

[3] Rathbone (1990), esp. 103–10, 122–4.

tainly neither Diodorus nor Josephus is remotely entitled to claim such authority. Rather, Bagnall and I started in the same way as do most historical demographers who have dealt with this subject: we reversed Rathbone's argument and placed primary emphasis on carrying capacity as setting a reasonable upper limit on the ancient population level; and so we plumped for an approximate population of about five million in the mid-second century CE, immediately prior to the onset of the great Antonine plague in 168 CE.

However, as a few reviewers have pointed out,[4] there are problems here. In particular, Bagnall and I found it difficult to fix Egypt's total urban population (comprising Alexandria, the Greek cities, and the *metropoleis*) at much less than 1.75 million, or about thirty-five per cent of a total population estimated at 4.75 million at its acme. As we observed, 'both figures seem high to us, but we lack means to test their accuracy'.[5] Our discomfort was most acute when it came to the implied ratio between the urbanized and rural populations, although admittedly even Egyptian *metropoleis* still supported a partially agricultural workforce.

Notwithstanding these difficulties, I continue to prefer a concept of carrying capacity as establishing a theoretical maximum level for ancient populations under the then prevailing social and economic conditions. Of course, I am assuming, for present purposes, that the concept of carrying capacity, which originated in animal biology, is sufficiently clear to be meaningful when applied to pre-modern human populations, though I realize that this assumption can be questioned; but I will not discuss the numerous uncertainties exposed in Joel Cohen's recent book on the subject.[6] Nor is it unlikely that some regions of the Roman empire, particularly in the West, never came close to reaching their theoretical maximums, either because they

[4] One of the most determined assaults on our reconstruction is made by Lo Cascio (1999a).

[5] Bagnall and Frier (1994) 56. Since this paper was written, Walter Scheidel has demonstrated to my satisfaction that pre-modern data support a higher Egyptian carrying capacity than Rathbone believed: perhaps 6.0 or 6.5 million (Scheidel (forthcoming a) ch. 3). If Roman Egypt's urban population is kept at about 1.75 million and its total population is reset at 6.0 million, the urban population drops to a more credible 30 per cent or so. This solution may be correct, but the real issue in my present paper is whether there is any good reason to believe that Roman Egypt's population was in fact approaching or surpassing the natural carrying capacity of the Nile valley.

[6] Cohen (1995).

were thinly settled to begin with (e.g., Roman Britain) or because they were caught up in chronic economic doldrums (e.g., Greece).

But for the central, more densely populated regions of the empire, what I want to explore is the possibility—*just the bare possibility*—that their populations were pressing upward against the maximums imposed by carrying capacity, and in some instances perhaps even surpassing these maximums. As I hope to show through indirect evidence, this is at least a possibility, one that some demographic evidence seems to favor, that we may be unable to rule out decisively, and that we are therefore obliged to bear in mind when evaluating Elio Lo Cascio's string of recent proposals on gross population.[7]

Let me clarify my starting point through reference to a well-known historical example, that of early modern France.[8] Thanks to several decades of spirit-crushing archival labor, the French demographic situation is now relatively clear for the half century before the Revolution in 1789, though the record becomes more fragmented and murky as one progresses backward from about 1740. In 1600, the French population (within present borders) is usually estimated to have been about 18 to 20 million. By 1700 it had grown very little, to 21.5 million, implying a long-term annual growth rate below 0.1 per cent at the most. The eighteenth century saw somewhat greater growth, particularly after 1750, and on the eve of the Revolution the French population stood at about 28.6 million; but the annual growth rate in the eighteenth century was still only slightly above 0.1 per cent. Even during the last fifty years of the *Ancien Régime*, France's growth rate was far lower than its neighbors'. In consequence the French population gradually diminished as a share of the estimated total European population, from about a fifth in 1600 to less than a sixth in 1789.

Why did France have difficulty sustaining growth during this period? Part of the answer came with the important discovery, by demographers at the Institut d'Etudes Démographiques, that France had far higher mortality rates (and especially much higher infant mortality) than did at least its northern neighbors. For example, between 1740 and 1789 French life expectancy at birth oscillated between

[7] Lo Cascio (1996a,b, 1999a,b).

[8] My discussion of French population relies on the authoritative summary of demographic results in Dupâquier *et al.*, eds. (1988). On gross population, see ibid. 52–68.

25 and 30 years, fully ten years below England, Sweden, and, as it seems, Germany.[9] This combination of high mortality and slow growth probably indicates that the French population was under considerable stress during the pre-revolutionary period.

Admittedly, demographic historians differ in evaluating this era of French history, and the crux of their differences is of some significance as we move to examine Roman demography. On one side stands the eminent French demographer Jacques Dupâquier, who hypothesizes, especially for the seventeenth century, 'un système homéostatique': a population that is largely self-regulating, and that meets each successive demographic crisis through internal mechanisms tending to restore numbers while avoiding excess growth. Although the homeostatic system apparently began to crumble during the first half of the eighteenth century, internal control of population growth remained characteristic of France even during the nineteenth century, so that birth and death rates remained very near balance until at least 1850.[10]

French self-regulation is a noteworthy historical achievement, but it requires further comment. Already in 1956, for instance, Alfred Sauvy had observed that pre-revolutionary France probably suffered from a population total far above the optimum level that its land area could sustain under then prevailing technological and economic conditions;[11] and subsequent historical studies have tended to confirm this observation not only for France, but for early modern Italy, Spain, and Portugal as well. Ferdinand Braudel, who reviews the evidence, unhesitatingly speaks of Mediterranean overpopulation already by 1600, when the basin's total population is estimated as sixty to seventy million—probably comparable to the Roman empire's, though far differently distributed, of course.[12]

The Industrial Revolution would eventually liberate most European populations from these material constraints on population growth.

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[9] On French mortality, A. Bideau, J. Dupâquier and H. Gutierrez in Dupâquier *et al.*, eds. (1988) 222–43. Of 1,000 newborns, an average of statistics from five early modern localities shows 715 survivors at age 1, 499 survivors at age 5, and 459 at age 10: ibid. 224. Gelbart (1998) has an interesting account of Louis XV's efforts to lower infant mortality. Comparative statistics on life expectancy at birth are taken from Wrigley and Schofield (1989) 246; the French life expectancies come from Blayo (1975).

[10] Dupâquier *et al.*, eds. (1988) 413–36.

[11] Sauvy (1956) 186–7.

[12] Braudel (1995) 394–418.

But for the *Ancien Régime* it is likely that France's dense population contributed to its unusually high mortality rates. In this connection, E. A. Wrigley and Roger Schofield, in their monumental study of the early modern English population, argue that France may have resembled a 'high pressure' demographic regime in the sense that 'population and mortality are high, population is large relative to available resources and growth is curbed principally by the positive check' of increased mortality.[13] However, as we shall shortly see, the French were not without social and cultural wherewithal in responding to the exigencies of this 'high pressure' regime.

One final point, before we desert France for a time. Like the Romans before them, the French of the *Ancien Régime* had limited knowledge of their own population, but they were chronically interested in the subject and attempted, as best they could, to count themselves. By the eighteenth century French intellectuals also widely subscribed to the fantastic notion that France was becoming depopulated in relation to its Roman population levels. In the early eighteenth century, for example, Montesquieu estimated France's population at only fourteen million persons, six million less (as he supposed) than *autrefois*; and coarse underestimates of this sort gained wide popular currency, even in the teeth of more accurate counts. The rationale underlying such low estimates was evidently political: the *philosophes* were convinced that France's corrupt royal government had led to an inevitable decline in population. The *philosophes* had become confused because they falsely supposed that population growth was not only an unqualified good in itself, but also an intrinsic attribute of a robust and prosperous society.[14] False assumptions of this sort are commonplace in the annals of popular demography, and we do well to bear them in mind when assessing ancient reports on population and depopulation in the Roman empire.

And now to Rome. Almost all ancient historians now accept the view, originally propounded by Keith Hopkins in 1966, that for the general population, average Roman life expectancy at birth is likely to have lain in a range from 20 to 30 years.[15] Such empirical evi-

[13] Wrigley and Schofield (1989) XXIV–V.
[14] On the philosophes, see J.-C. Perror in Dupâquier *et al.*, eds. (1988), esp. 511–8; also Dupâquier, ibid. 56–7 ('Le mythe de la dépopulation et ses ravages'), who provides the Montesquieu quotation from *Lettres Persanes.*
[15] Hopkins (1966) 263–4.

dence as we have, though it leaves much to be desired, tends to support this view, albeit with some qualifications. This evidence includes the apparent estimates of life expectancy in a famous schedule of the tax of annuities, called 'Ulpian's life table'; the distribution of ages at death both in the funerary inscriptions from Roman North Africa (where selectivity in giving age is not a problem, unlike in the European epitaphs) and in the few late imperial cemeteries that palaeodemographers have accurately analyzed; and the age distribution in the Egyptian census returns.[16]

This empirical evidence is far from ideal, but the best we have. One point must be made regarding it. The evidence appears fairly uniform—perhaps even surprisingly uniform—in supporting a population that resembles the lower levels of the Coale-Demeny Model West.[17] Levels 1 to 3 of Model West seem the likeliest. This resemblance, however, is not entirely problem-free. Above all, the empirical evidence relates only to adolescent and adult mortality, from about age 10 onward. What the evidence supports is a population in which average life expectancy at age 10 was approximately 35 to 37 years. This figure may be taken, I think, as relatively secure for the general Roman population, with an exception being made only for the affluent elite whose life expectancy may well have been considerably higher.

On the basis of these general adult mortality figures, the Coale-Demeny models permit us to restore corresponding levels of infant and child mortality. Unfortunately, however, this aspect of the standard models is controversial because the broad statistical correlation between levels of infant and adult mortality is notoriously unstable in pre-modern societies. Further, there is at least some reason for fearing that because of the statistical method Coale and Demeny used in constructing their lower models, they may systematically overestimate the 'normal' level of infant mortality. A few Roman cemeteries apparently do indicate, what would in any case not be surprising, high levels of infant mortality, but the pattern is insufficiently consistent for confidence. This is a serious deficiency in our knowledge, since infant mortality has major implications not only for life expectancy

[16] This evidence is summarized in Bagnall and Frier (1994) 109–10.

[17] Model life tables are cited from Coale and Demeny (1983). Use of Coale-Demeny tables is discussed in Bagnall and Frier (1994) 31–8 and 76–81.

at birth, but also for the fertility rates that are required for population replacement or growth.[18]

But if we hypothetically accept the Coale-Demeny values for infant and child mortality, then restored Roman average life expectancy at birth should be on the order of 20 to 25 years[19]—not only in the lower half of the Hopkins range, but also substantially below France in the mid-eighteenth century, and indeed only marginally higher than the life expectancies usually attributed to anthropological populations. As should by now be obvious, such an estimate cannot be accepted without considerable caution. On the one hand, infant mortality remains a puzzle; for instance (and this is by no means an impossibility), were we to restore, for children under age 5, mortality rates one-sixth lower than those in the Coale-Demeny models, Roman average life expectancy at birth would be about three years higher. On the other hand, the Roman empire embraced an extremely large and geographically diverse territory, and considerable regional variations and temporal fluctuations in mortality are definitely to be anticipated, even if evidence for differences is at present still outstanding. Still, it seems safe to posit relatively high levels of Roman mortality, and the question then becomes how these high levels are to be explained.

A first approximation to answering this question may involve seeking explanations in what can be termed the demographic environment: factors 'external' to the demographic regime that may have tended to promote elevated mortality. Such factors might include, for instance, the deficiencies of Roman medicine and sanitation; the severe maldistribution of income and wealth, leading to poor nutrition in the general population; patterns of heightened urbanism that facilitated the transmission of disease; and the bureaucratic inadequacy of Roman government in combating both the endemic and epidemic causes of mortality. While the collective force of such factors was doubtless considerable, it is admittedly impossible to isolate and quantify the potential contribution of each one.

In any case, it is on point to observe, just here, that such 'external' causes may be insufficient as a complete explanation, particularly

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[18] On the fluctuating relationship between infant and adult mortality, see Livi-Bacci (1991) 72–8; Woods (1993).

[19] Of 1,000 newborns, Model West Level 1 Females posits that 635 females survive to age 1, and 468 to age 5; Level 3 Females posits that 694 females survive to age 1, and 544 to age 5. See Coale and Demeny (1983) 42–3.

granted the presence of other 'external' factors, such as the long imperial peace or the stupendous efforts of Roman engineers in managing the supply of fresh water to cities, that might have worked in the opposite direction. At this point I turn to some aspects of modern demographic theory that suggest a quite different sort of explanation for high Roman mortality.

In general, it is probable, as Wrigley and Schofield have recently observed, that 'prior to the industrial revolution population growth was necessarily very slow or non-existent because the capacity of a society to increase the level of material production was slight if present at all'; and, in consequence, 'birth and death rates were necessarily at closely similar levels in the long term, though not necessarily in the short'.[20]

In this context, Wrigley and Schofield propose two limiting cases that merit closer scrutiny. The first is the so-called 'west African' situation, in which 'mortality was always high because the disease environment was so unfavorable . . . In such circumstances high fertility was essential for a population to survive; in this sense high mortality could be said to have 'caused' high fertility'. The second limiting case is the 'Chinese' situation, in which 'the disease environment was less deadly but social conventions made early and universal marriage mandatory. As a result, fertility was high and, because rapid growth had to be short-lived, mortality was high too. In the 'Chinese' case high fertility 'caused' high mortality'.[21]

Considered as ideal types, these two limiting cases have the common theoretical feature that an external circumstance (a hostile disease environment, or a social convention of early and universal marriage) is described as 'causing' a demographic feature (high mortality or high fertility, respectively) that then becomes more or less determinative of the demographic regime as an entirety: 'west Africa' is a mortality-driven regime, while 'China' is a fertility-driven regime. The latter portion of this hypothetical causal sequence is of particular interest, since it uses an 'internal' factor to explain a prominent demographic feature of each case.

[20] Wrigley and Schofield (1989) XXIV.

[21] Ibid. XXIV. These limiting cases are abstractions based on what is commonly believed about pre-modern West Africa and China. However, Lee and Wang (1999) argue that the 'Chinese' scenario may not apply to historical China.

Even though France of the *Ancien Régime* may well have suffered from severe overpopulation, in two respects its case only partially resembles the 'Chinese' situation. First, pre-revolutionary France, like most 'northwest European' nations, did not have a social convention of early and universal marriage; women usually married comparatively late, in their mid- to late twenties, and, as is normal in 'late marriage' populations, the incidence of permanent female celibacy was appreciable. Both factors doubtless contributed to the remarkable homeostasis that France appears to have achieved during the seventeenth century, and this homeostasis in turn prevented mortality levels from rising still further.[22]

Second, during the eighteenth century, after traditional homeostatic controls had apparently deteriorated, some portions of the French general population resorted to an entirely novel control on population growth: through the use of contraception, couples in some areas of France initiated more or less systematic family limitation within marriage. This cultural change in age-old patterns of family life ushers in the earliest stages of the fertility transition, an historic event that, perhaps more than any other, has fundamentally influenced the formation—or, more accurately, the transformation—of the modern family.[23]

From the perspective of historical demography, what is at play in this discussion is also the metamorphosis of a long-standing dogma of the discipline. Still today, ancient historians frequently operate under the impression that high mortality levels, such as those that we now associate with the early Roman empire, will inevitably have required extremely high fertility rates if population replacement was to occur. However intuitively plausible this proposition may seem, it is in fact untrue. Even at levels of mortality that we would today regard as exceedingly high, virtually all human populations retain considerable reserves of unused reproductive capacity. That is to say, control of population is and has always been an ordinary part of socialized human life.

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[22] On age of marriage and celibacy, F. Lebrun in Dupâquier *et al.*, eds. (1988) 303–6; on marriage as the critical element in homeostasis, Dupâquier, ibid. 427–34.

[23] The French evidence is discussed by A. Bideau and J.-P. Bardet in Dupâquier *et al.*, eds. (1988) 373–99. For a good account of the modern fertility revolution and the theoretical problems associated with it, see Alter (1992).

I do not mean to imply, of course, that there have not been dramatic historical changes in the structure of population control. In a famous essay originally published in 1978, Wrigley observed:[24]

> When the demographic transition occurred it did not take the form of a move from a situation in which fertility was uncontrolled to one in which it was reduced by the exercise of prudential restraint. Fertility is under constraint in almost all societies. (. . .) The key change was from a system of control through social institution and custom to one in which the private choice of individual couples played a major part in governing the fertility rate. (. . .) Whether choice is likely to be a solution which a society may countenance will be greatly influenced by its basic mortality schedule. At, say, level 1 model West a population could hardly allow private choice since it must mobilize maximum fertility if it is to survive at all.

Wrigley argues that the demographic transition describes an historical movement from social to individual control of fertility, and this point is certainly correct. He also argues that, to some extent, the movement is made possible by an amelioration in the basic mortality schedule. But this second proposition is more problematic at least when it speaks of high mortality populations being obliged to 'mobilize maximum fertility'. In this respect, a good and well known example is rural China in the early 1930s, which, as a classic study showed, had a mortality rate comparable to Rome's (an average life expectancy at birth of about 22.5 years), but which nonetheless successfully replaced itself with marital fertility rates that were less than half of the highest rates known to be socially sustainable.[25]

In sum, 'mobiliz[ing] maximum fertility' was rarely, if ever, a principal social characteristic or goal of pre-transition populations, even those with very high mortality. Indeed, quite the reverse. As Ansley Coale has put it,[26]

> The statement that traditional societies developed customs that promoted high fertility, or faced extinction, should . . . be amended to say that traditional societies developed customs that kept fertility at *moderate* levels, avoiding both fertility so low that negative growth would make the population shrink to zero, or so high that positive growth would lead to an overcrowded habitat, and hence to higher mortality, and greater vulnerability to catastrophe or rival groups.

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[24] Wrigley (1987) 209.
[25] Barclay *et al.* (1976).
[26] Coale and Watkins, eds. (1987) 7 (Coale's emphasis).

This change in demographic perspective should alert us to what is, in truth, my central theme. Simply put, what I wish to observe is that the Roman empire was clearly exposed to the possibility of excess population growth, and that as a result some portions of it could have approached the 'Chinese' situation described by Wrigley and Schofield. In the 'Chinese' situation, high fertility rates, induced by social conventions of early and universal female marriage, lead gradually to high mortality rates. I am not asserting that this actually occurred in the Roman empire, or in any of its regions. My contention does not take the form of a thesis, and I doubt that it can even be described as rising to the level of an hypothesis. All I am saying is that we, as social historians, should be keenly aware of the possibility of overpopulation, particularly if we accept, as Roman social historians now generally do, levels of mortality as high as those that were conjectured by Keith Hopkins and are now supported by such empirical evidence as is available.

What were the constraints on general population growth during the early Roman empire? We may start examining the two main methods that were employed in pre-revolutionary France: delay of female entry into marriage, and the use of contraception or induced abortion to limit family size. Both these methods are reasonably effective, but they operate differently. The first is a 'starting' strategy in which women do not marry and become regularly sexually active until well into the period in which their fecundity is at its peak; the second is a 'stopping' strategy that seeks to halt procreation when a couple reaches what they regard as a sufficient number of children. I concentrate on fertility in legitimate marriage because, as I have shown elsewhere,[27] married free women clearly sustained the overwhelming preponderance of the reproductive burden in the Roman world; for instance, even if the slave population was successfully itself, the general fertility rates of adult slave women must have been far below those of married free women.

In a recent study, I reconstructed the pattern of female age at first marriage for Roman Egypt.[28] The mean is approximately 20, implying a marriage rate that is, I should stress, early but not exceptionally rapid by pre-modern standards. Though female marriage begins at age 12, by their fifteenth birthday only just over 12 per

[27] Bagnall and Frier (1994) 144–5.
[28] Bagnall, Frier and Rutherford (1997) 105.

cent of free women are married in the reconstruction. The bulk of female marriage occurs between ages 15 and 20; by their twentieth birthday more than 60 per cent of women are married, and by their twenty-fifth over 85 per cent. Using data on funerary dedications from Brent Shaw and Richard Saller, I was able to reconstruct a virtually identical pattern for female marriage in the Western Empire; however, because funerary dedications are always strongly influenced by latent cultural influences, this body of data is undoubtedly less reliable than the Egyptian returns and should be interpreted as only confirmatory.[29]

What these reconstructions indicate is that most free Roman women were married during their most fecund period, between ages 20 and 30. That is, the Romans did not employ a 'starting' strategy of delayed female marriage in order to control population growth. There is nothing surprising about this conclusion, of course; the Roman pattern is reasonably well known by now, although as of yet its broader cultural implications have perhaps not been fully explored.

The second major means of population control is family limitation through contraception and abortion. John Riddle's recent book on pre-modern birth control has moved this subject once more to the front burner.[30] In brief, Riddle maintains that ordinary Romans married women widely limited their families by controlling fertility through drugs that were both readily available to them and effective in achieving contraception or abortion. Both in our book and in a subsequent article,[31] I have discussed in some detail the principal reasons for rejecting Riddle's thesis. Riddle, however, still finds my arguments unpersuasive; accordingly, we have now a new book from him, once more under the prestigious imprimatur of Harvard University Press. Its title is *Eve's herbs*.[32]

My arguments against Riddle's thesis are publicly available, and I will not rehearse them at length here. As historical demographers have long recognized, in all adequately known populations before the modern fertility transition, the pattern of fertility among married

[29] The data were presented in summary form by Shaw (1987a); see also Saller (1994) 25–41.

[30] Riddle (1992).

[31] Bagnall and Frier (1994) 135–55; Frier (1994).

[32] Riddle (1997). From a demographic perspective, this book is even weaker than its predecessor; the specifically demographic sections (12–21 and 169–79) are a compendium of common demographic fallacies. (Cf. also above, Chapter 1, n. 160.)

women[33] is affected by two main variables, which operate largely independently. The overall *age distribution* of marital fertility is determined, virtually entirely, by adult female fecundity: marital fertility rates decline gradually in the 20s and 30s, and then steeply in the 40s, as an all but direct function of declining fecundity as women age. This age distribution of marital fertility is rather unfortunately termed 'natural fertility', by which is meant 'fertility which exists or has existed in the absence of deliberate birth control' to limit family size.[34]

By contrast, in pre-transition populations the overall *level* of female fertility is determined by social practices that act, with disparate effectiveness depending on exactly how they are used, to restrain this fecundity irrespective of parity, the number of children that women have previously borne. That is, whatever causes prevent marital fertility in most pre-transition populations from attaining the maximum levels that are socially sustainable, these causes have an approximately equal impact across the entire span of adult female fecundity, from menarche to menopause. These causes 'dampen' but do not significantly alter the basic 'natural' age distribution of marital fertility, and this is true even where the 'dampening' effect is enormous, as with rural China in 1930.

To judge from ancient medical writings and other literature, the Greeks and Romans were fascinated by the reproductive process and by contraception and abortion as means to control it. Further, Riddle may be correct in arguing that some methods prescribed by ancient sources would be effective if used in accurate dosages. However, if Riddle were also correct in contending that ordinary married couples generally employed these methods to limit family size, then we would predict a significant decline in the rate of legitimate births as women age and the desired family size is reached.[35] In this respect, Rome would anticipate the modern fertility transition.

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[33] This limitation deserves emphasis: only marital fertility (childbirth within legitimate marriage) is at issue here.

[34] This is the famous formulation of Henry (1961) 81.

[35] As couples begin to use methods of birth control in order to plan their families, the following typically occurs: births are concentrated in the early years of marriage; intervals between births increase as women age; and the average age of last maternity declines sharply. These results are visible in gross statistics, but not always in the experience of individual families. It should be stressed that the introduction of family planning, and even of family limitation, need not immediately

However, the only available ancient data, which come from the Egyptian census returns, show quite clearly that the predicted dropoff does not occur. My Coale-Trussell reconstruction of Egyptian marital fertility rates, based on 188 cases in which a mother's age at childbirth is attested, demonstrates that Egyptian marital fertility closely conforms to the usual age distribution of marital fertility before the fertility transition (the 'natural fertility' pattern).[36] Reproduction continues at the 'natural' rate throughout the period of female fecundity, until women are in their 40s—a pattern inconsistent with family limitation. On the whole, therefore, we are probably justified in concluding that active use of contraception and abortion was largely confined to non-marital sex, though family limitation among the elite classes certainly cannot be ruled out.

In short, at least on present evidence, there is no reason to believe that ordinary Roman married couples successfully limited their families through contraception and abortion. If in fact these couples had access to reliable means for effecting birth control, then either they simply failed to consider family limitation as a possibility, or they considered and rejected it because they did not want to limit their families.

Despite its patent defects, the Riddle thesis has at least the virtue of focusing attention on the issue of population control in the Roman world. Exactly how (if at all) did the Romans avert a population explosion? This is a difficult question because our sources leave us largely in the lurch; we know little about the social practices that may have acted to restrain marital fertility from edging upward toward its maximum sustainable levels. Ancient medical writings, combined with Egyptian wetnursing contracts, indicate the existence of two practices that are also widely found in other pre-transition populations: a protracted period of postpartum sexual abstinence that was usually justified through folk beliefs about the adverse effects of semen on lactation.[37] How widely and effectively these practices were observed is unfortunately a matter of conjecture.

What is not entirely conjecture, however, is another pattern that emerges from my reconstruction of female marriage in Roman Egypt.

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result in lower overall rates of fertility, though this has been the general outcome in the modern world.

[36] Coale and Trussell (1974, 1978); Bagnall, Frier and Rutherford (1997) 110.

[37] See Bagnall and Frier (1994) 148–51.

Marriage among ordinary Romans was fragile for two reasons: first, high mortality meant that spouses quite frequently died; second, divorce was a private matter, relatively readily available on demand to either party and, as the Egyptian census returns show, also made use of by the lower classes. Women who were widowed by the death of a spouse, or who were divorced, did remarry, but increasingly infrequently as they aged; the census returns offer no secure case of female remarriage after age 35. As a consequence, the number of free women who are still married declines from a peak around 80 per cent for women in their late 20s, to around 35 or 40 per cent by age 50. Comparable patterns are not uncommon in pre-transition Mediterranean populations; and both Richard Saller and Jens-Uwe Krause have recently argued, independently and on a more theoretical basis, against the assumption that after the loss of a spouse through death or divorce, fertile females inevitably remarried in the populations of the Roman empire.[38] On the other hand, although the failure of women to remarry may have had some depressing effect on overall fertility levels, historical demographers have repeatedly cautioned that this effect is unlikely to be large, mainly because of declining female fecundity after age 35.

My inventory of social constraints on marital fertility is certainly not exhaustive.[39] Indeed, any customary or religious constraints on sexual intercourse between husbands and wives are also likely to be germane, as is any evidence that males were seasonally absent from their households. Brent Shaw's paper may shed some additional light on this subject, if in fact a seasonal pattern of births can plausibly be associated with cultural patterns of sexual contact (above, Chapter 2). And perhaps we can even redeem Riddle to some extent by speculating on a limited role for abortion and contraception within marriage, though not for family limitation but rather to ensure a safe interval between successive births.

In any case, I cannot sufficiently underscore the importance of this issue for Roman social historians. Unfortunately, and this point merits emphasis, we have at present no reliable means of determining how high Roman marital fertility rates actually were. That is, although we can fix the age distribution of marital fertility with

[38] Saller (1994) 68; Krause (1994–95) vol. 1.

[39] For instance, I omit discussion of infanticide, a 'post-natal' form of fertility control that is more usually counted as part of infant mortality.

some considerable confidence, we are at present unable to establish its level. In my *Classical Philology* article, I reconstructed the levels of Egyptian marital fertility rates on the assumption that the Egyptian population was only slightly more than reproducing itself. These reconstructed rates are approximately 70 per cent of the maximum that historical demographers believe to be socially sustainable; and on the argument that I am making, this 'dampening' effect from the theoretical maximum fertility rates is entirely or nearly entirely attributable to non-parity-related social practices of fertility control within marriage.

There is certainly nothing implausible about this reconstruction; indeed, it falls towards the low end of the ordinary range in marital fertility rates that are attested for pre-transition populations. The problem I am raising is, in fact, exactly the opposite: what if my reconstruction errs in underestimating marital fertility rates, so that the Egyptian population was actually much more than reproducing itself? This certainly cannot be considered impossible on present evidence, and I am grateful to Elio Lo Cascio for leading me to consider the question more seriously.

In the *The Demography of Roman Egypt* Roger Bagnall and I conjectured an intrinsic growth rate of about 0.2 per cent per year, which leads to very slow population growth (doubling every 3.5 centuries).[40] This long-term rate is close to the norm for most pre-transition populations, and it would probably be enough to provide a cushion for dealing with crises of mortality. But the Egyptian data are admittedly not inconsistent with a higher growth rate: say, 0.5 per cent (with doubling every 140 years). The more basic issue, then, is whether such higher growth could possibly have been sustained in the long term without serious negative effects on mortality levels—exactly the effects we may in fact be observing if the standard interpretation of the Roman mortality evidence is correct.

This is also the core of the 'Chinese' situation that Wrigley and Schofield proposed as a limiting case. Their scenario for the 'Chinese' situation is overtly Malthusian: a rising population, as it outstrips available resources, gradually leads to increased mortality rates as a positive check on population growth, until a kind of demographic equilibrium is established at a level of mortality that is sufficiently

[40] Bagnall and Frier (1994) 81–90.

high to counterbalance the upward surge in population. The adverse consequences of such a scenario are apparent: the resulting population is thereby rendered simultaneously more impoverished and more vulnerable to the onset of catastrophic plagues. More is worse.

In this paper, I have tried to present some of the evidence for believing that large portions of the Roman population may have suffered from considerable demographic stress. There is other evidence that I have not considered. For example, Richard Duncan-Jones has documented a considerable rise in prices for Egyptian land and wheat during the first two centuries of our era.[41] This rise is ominous if, as seems not improbable, it points to a decline in real wages among the general Egyptian population; but the evidence is not incontestable, and Dominic Rathbone has suggested that no clear price trends are in fact detectable.[42]

However, rather than reviewing additional evidence of this sort, I want to conclude with three broader points. First, let us hypothesize, just for the sake of argument, that portions of the Roman empire did suffer from overpopulation during the Principate. In the absence of reliable sources for actual population levels and changes, are we able either to confirm this hypothesis, or to rule it out categorically, on the basis of other indicia both literary and archaeological? Is it possible that so momentous an event could simply pass us by? This problem has long troubled me. For the Roman world, as for pre-modern societies generally, when population levels are known from reliable data, much other circumstantial evidence falls gradually (if often still controversially) into place within a broader causal framework; but in the absence of such accuracy, circumstantial evidence—such as Egyptian grain prices—often remains incurably ambiguous and thus susceptible to diverse interpretations.

I see no easy solution to this problem, but I suspect that as historians we now do better by concentrating on population change rather than level, and by paying closer attention to the results of field survey archaeology, especially if that discipline eventually succeeds in establishing criteria for the verifiable analysis of its results. What we are looking for are the numerous indirect indicators of population growth: occupation of new territory, clearing of new land, agricultural improvements, urbanization, and emigration; and more

[41] Duncan-Jones (1990) 143–55.
[42] Rathbone (1996, 1997).

particularly the geographical distribution of these indicators within the Mediterranean basin.[43] Much has already been done along this line, to be sure, but the *Annales*-inspired synthesis still lies far in the future.

Second, of course I have not meant to imply that rising population must inevitably lead to the 'Chinese' scenario. Within the rich literature on the economics of development, abundant alternatives are available. For example, Ester Boserup has long argued that population pressure has often been historically responsible for changes in technology and patterns of land use that eventually raise the economic productivity of labor while bringing about more intensified forms of cultivation and land-augmenting investment, including drainage of swampy lands and irrigation for multiple cropping.[44] For their part, Dominic Rathbone, Dennis Kehoe, and other Roman historians have recently applied such economic thought to the surviving evidence for agricultural practices in Roman Egypt and elsewhere.[45] This line of research obviously has much to recommend it, and the results have thusfar been impressive. At the end of the day, however, the real question about such an approach is primarily moral: how comfortable can we ever feel in arguing, on the basis of present evidence for the early Roman empire, that increased rationalization and technological development could possibly have sufficed to offset any major surge in population levels?[46]

[43] Cf. D. Coleman in Coleman and Schofield, eds. (1986) 31–2 (citations omitted): 'Most models of human population regulation envisage at least some temporary environmental limit on population size imposed by climate and technology. Beyond the maximum population, resources for subsistence may degrade from overhunting, soil erosion, laterization of ancient soils in tropical areas, desertification and other responses to overcropping [;] while such changes are in progress—and they may last a long time—the population will have overshot its carrying capacity and its numbers will be living on borrowed time before inevitably falling to a lower density. (. . .) Late Imperial Roman populations (. . .) have been claimed as historical examples of such pressures, as well as many contemporary tropical populations, especially in the arid tropical zones of Africa.'

[44] See esp. Boserup (1981). On the relationship of Boserup's theories to classical Malthusianism, see Lee (1986).

[45] Rathbone (1991); Kehoe (1992, 1997).

[46] To put this point differently: It is predictable that any appreciable increase in population will result in technological innovation, increased rationalization, and more intense use of scarce resources, at any rate if the population attempts to maintain its standards of living as defined by per capita real income. Therefore the mere observation of the occurrence of such phenomena in the Roman empire is insufficient in itself to determine whether they transpired at a pace adequate to offset the population increase.

But this leads me to my third point. In 1984/85, when I had the pleasure of visiting Cambridge while on a sabbatical from Michigan, a sort of grand seminar was in progress, with growth in the Roman economy as its theme. The wine was good, but the papers were also scholarly and impressive, and many of them linger in my memory. Nevertheless, what struck me most deeply then was that not a single paper incorporated any reference to demography as an integral element in assessing and understanding the development of the Roman economy. It was not just that population was not expressly bracketed out of consideration. Population simply disappeared as an issue: no models, no assumptions, no nothing.

I am still at a loss to explain why this should have been so, particularly since, for pre-modern societies, the centrality of population issues has long been recognized in the social disciplines and the allied historical fields from which we commonly draw our models and our inspiration.[47] Perhaps the explanation lies in the enduring legacy of Moses Finley, whose voluminous writings on the ancient economy only rarely refer directly to problems of population. Or perhaps it was just the more general consequence of an ingrained scholarly despair as regards our ability to say anything both new and interesting on this pesky topic.

In any case, whatever the reason, such abstinence was a strategic error of the first water. If we assume, as assume we must, very low levels of per capita income in the early Roman empire, it is nonetheless true that population growth could well have contributed far more than any other single source to any overall increase in the Roman empire's economy during this period. Indeed, to a large extent the wealth of the Roman empire was simply a function of the enormous size of the population under its control,[48] a fact that may be sufficient in itself to explain the pronatalist policies of the Caesars; for no matter how impoverished the general population was or became, the central government could still be confident of abstracting a surplus sufficient for the empire's administration and defense.

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[47] See, e.g., Ghatak (1995) 231–65.

[48] On a conservative estimate, the Roman empire at its height controlled about one-fifth of the earth's population, concentrated in approximately three per cent of its land area. The contemporary Eastern Han empire was remarkably similar both in land area and population.

The danger in such population growth, however, was that the empire, or large portions of it, might eventually have fallen into what developmental economists refer to as a population trap, in which the rate of growth of population exceeds the rate of growth of real income, so that per capita real income begins to decline. The burden of my paper has been to suggest that the Roman empire may in fact have encountered such a population trap. However you feel about the plausibility of this suggestion, I would ask you, in your research, at least to bear the possibility in mind.

CHAPTER FIVE

URBAN POPULATION IN LATE ROMAN EGYPT AND THE END OF THE ANCIENT WORLD

Richard Alston

Introduction

Since Pirenne, the seventh and eighth centuries have been seen by many as a period of transition from the ancient to the medieval world.[1] Changes in the settlement pattern in the Near East, especially in Syria, Arabia and Asia Minor, have encouraged historians to posit an economic and demographic crisis in this period, but, as with all other historical boundaries, there is disagreement as to the nature of the transition, its timing, cause and; moreover, whether the evident continuities outweigh the changes.[2] I argue here that there was a significant demographic transition in Egypt in the centuries surrounding the Arab invasion. This transition does not rival the modern demographic transition in terms of its radical effects on the structures of population (life expectancy, perinatal mortality, gross population, etc.) but is sufficiently striking to be compared with other pre-modern demographic shifts, such as that of the fourteenth century. This ancient transition cannot be attributed to the Arab invaders, the closing of the Mediterranean to trade, or the advent of bubonic plague in the sixth century, though all these factors may have contributed. Instead, I suggest that a fundamental transformation in social and economic organisation is the only likely explanation for the changes in settlement pattern observed. Of necessity, the material

[1] Pirenne (1925, 1939).

[2] Foss (1976, 1977, 1979) argues for marked urban decay in the seventh century in reaction to the Persian invasions. Trombley (1997) argues a similar point for Syria. Roueché (1989) XXVI places the transition for Aphrodisias in the mid sixth century. Carver (1996) argues for continuity. Therbert (1983) argues that the evidence for decline in Africa in the Arab period is mixed, but there is no evidence to suggest urban decay before the Arab invasions. Charanis (1961, 1966) argues for continuity into the sixth century but marked settlement change in the following centuries.

I present will provide only limited confirmation of the hypotheses suggested and the scope of this essay means that, for instance, sections on the demographic and social patterns of the Roman period must over-simplify a complex historical situation. Although the demographic changes in this period were dramatic, the surviving material is insensitive to the subtle alterations in socio-economic structure that produced that transformation. In addition, the causal connection between observable changes in socio-economic structures and demographic change on the scale suggested cannot be established absolutely but, as I shall argue, there is no workable alternative explanation.

Modelling Roman Settlement Systems in Egypt[3]

As in most other pre-industrial cultures, the majority of the population lived in villages, but these were not uniform in economic function or in size.[4] The smallest settlements were probably little more than a few houses and may have had populations of under fifty. There was a tendency to subsume the administration of such places into that of larger villages and this may reflect economic or cultural relationships between the various settlements.[5] The economies of these smaller villages were almost certainly completely agricultural. Larger villages, judging from papyri and archaeological material from the Fayum, were also predominantly agricultural settlements, with probably over ninety per cent of the population involved in farming.[6] In Karanis (in the northern Fayum), for example, there was only limited trade and craft production within the village and a small volume of trade with neighbouring villages. The economy of the village was at least partially monetarised and Karanis was neither economically nor socially isolated. Agricultural produce was traded in market centres or exported as taxes and rents, either in kind or cash. A large proportion of this exported produce ended up in the urban

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[3] Much of the argument here is presented at greater length in Alston and Alston (1997) and Alston (1998).

[4] For the demographic implications of the settlement system, see Rathbone (1990).

[5] See *P.Thmouis* I 99 for three closely related villages. Karanis subsumed the neighbouring villages of Kerkesoucha, Ptolemais Nea and possibly Psenarpsensis for some administrative purposes: *P.Mich.* VI 372; *P.Ryl* IV 601; *SB* XII 11105; 11109; see Geremek (1969) 14–25 for a full discussion of this issue; cf. Bagnall (1985). Other villages may have had a similar local importance, see Gallo (1992). *P.Tebt.* II, pp. 54–6, Tait (1992), and *P.Tebt. Tait.* on the villages of the southern Fayum.

[6] Bagnall (1993) 127–30.

markets, either in Ptolemais Euergetis (the local city), Alexandria, or possibly other centres of population. Although Karanis appears to have been a particularly large village with a population of between 1,900 and 2,300 in the second century, it seems likely that the economies of many other villages were essentially similar.[7] The notable extension of elite landholdings in villages that is general believed to have occurred during the third and fourth centuries probably helped further integrate the economies of the villages and cities.[8]

A third level of settlement may have had a rather more variegated economic base. Certain villages seem to have operated as market centres, perhaps as intermediate markets between the agricultural villages and the cities. These villages were predominantly agricultural settlements, though craftsmen and traders were a significant proportion of the population, probably more than fifteen per cent. There is no evidence on which to base population estimates for these villages.[9]

As with all other areas of the ancient world, there is no series of reliable statistics for the size of the population of the various urban communities. We can, however, estimate the population of five *metropoleis* in different periods, as in Table 1.[10]

Table 1: Estimated populations of Egyptian settlements

City	Document	Date	Estimated Population
Hermopolis Magna	*Stud.Pal.Pap.* V 101	*c.* 275	58,429
Ptolemais Euergetis	*Stud.Pal.Pap.* IV pp. 58–83	72–3	27,071
Thmouis	*PSI* III 111	2nd c.	24,564
Oxyrhynchus	*P.Oslo.* III 111	235	21,000
Oxyrhynchus	*P.Oxy.* XL 2892–5	270–2	12,087
Oxyrhynchus	*P.Oxy.* VI 908	199	11,901
Apollonospolis Heptakomias	Archiv. VI 427	116	8,784

[7] Alston (1995) 229, n. 20; Boak (1955b); Rathbone (1990) propose essentially similar figures though van Minnen (1994) argues for a much higher population.

[8] Rowlandson (1996) 102–24, 281; Bagnall (1992); Bowman (1985). The problem lies not in assessing the landholdings of the urban elite but in establishing whether there was a process of concentration of land in the hands of that elite.

[9] See Alston and Alston (1997) for a discussion of this type of settlement.

[10] Rathbone (1990) estimates the population for various urban and rural settlements but these figures are based on data collected in Alston and Alston (1997).

The diversity of estimated population levels, even for a single settlement, is not a cause for concern (it is anyway arguable that the estimates for the population of Oxyrhynchus from 199 and 270–2 are likely to be conservative while that for 235 is likely to be too high), since although the figures presented in Table 1 give a spurious impression of precision, the margins for error are such that these estimates can only provide us with an order of magnitude. The population figures range between the extremes of Apollonospolis Heptakomias and Hermopolis Magna, but even Apollonospolis Heptakomias appears to have been a substantial urban settlement in pre-industrial terms, though, of course, all these cities will have been dwarfed by megalopoleis like Alexandria (at around 500,000) and Rome.[11]

Like most other ancient cities, the Romano-Egyptian city was the home of the political elite and almost certainly the most wealthy residents of the region resided there. Taxes and rents flowed into the city, probably to an increasing extent throughout the period of Roman domination.[12] The city also appears to have been a significant centre or trade and craft production with twenty-five per cent or more of the population being supported primarily through crafts and trading. The cities were far more cosmopolitan than villages and social, political and economic networks (extrapolating from the evidence from Oxyrhynchus) stretched far beyond the boundaries of the particular nome. Striving for precision is somewhat futile, but, judging from surviving records of the network of communications, numismatic and archaeological evidence, Romano-Egyptian cities seem to have been at the centre of quite extensive trade networks.[13] In contrast to more primitivist models of the ancient city, I suggest that the prosperity of Egyptian cities was generated by integrated networks of urban and rural communities that combined to form a series of overlapping settlement systems. These networks allowed economic specialisation and the development of what seem to have been quite sophisticated urban market-systems which in turn provided an outlet for rural produce.[14] Trade, together with the political and eco-

[11] Rathbone (1990), Abbadi (1988), and Delia (1988) agree on an approximate level for the population of Alexandria, though all, especially Rathbone, express some scepticism as to the value of the primary evidence.
[12] See n. 8 above.
[13] Alston (1998).
[14] Not all markets were urban. Jördens (1995) argues for a multi-faceted and

nomic influence of the urban elite, supported large urban populations both in absolute numbers and relative to the rural population of Egypt.[15] The urbanisation of Egypt in the Roman and Byzantine periods manifests and generated prosperity both for urban and rural communities. These favourable economic circumstances encouraged changes in settlement pattern in the early Byzantine period as discussed in the next section.[16]

Settlement Patterns and Demographic Change from 300 CE

Population trajectory in the third century

Many periods of Romano-Egyptian history have been seen as encompassing dramatic demographic decline. A traditional view saw crisis in the early first century CE, though this has now largely been discredited.[17] There is substantial evidence of population decline in the second century. The gradual depopulation of various Mendesian villages over the middle third of the second century and what appears to have been a sudden forty per cent fall in the population of Karanis, supported by evidence of population decline at Soknopaiou Nesos, are dramatic indications of the problems of the period.[18] Although

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complex economy, as presupposed here, concentrating on the evidence for rural, regional markets in transport animals.

[15] The pattern of settlement suggested is somewhat similar to the integrated urban system suggested for Italy by Morley (1996, 1997), which builds on work on the city in other periods, such as Rozman (1976). See also the application of mathematical modelling to urban networks in Rihll and Wilson (1991).

[16] Increasingly, development economists have laid stress on the 'urban-rural interface' and the role of small towns in promoting economic development. See Baker and Pedersen (1992). The role of the city as a generator of prosperity has caused much modern debate. Morley (1996) 13–32 argues that the debate between those who see urbanism as a 'progressive force' and those who see it as parasitic on the ancient economy is unresolvable, though opts for a moderately optimistic view. An extreme case of the economic effects of urban growth is seventeenth-century London: see Wrigley (1987) 142–93, who describes London 'as a potent engine working towards change in the century 1650–1750' (156). See also the extended discussion of markets in Braudel (1982) 25–230, which, apart from the discussion of stock exchange and high-level financial facilities for long-distance commerce, offers interesting parallels with our period.

[17] Milne (1927); Bell (1938); Hanson (1973, 1988).

[18] Rathbone (1990). Bagnall and Frier (1994) 173–7 suggest that the plague had a significant effect on the demography of Egypt but are unsure as to what combination of factors explains the slight change in population structure. See also

population falls as registered in tax lists may substantially exaggerate the real demographic situation, especially if we believe that the rural population tended to be relatively mobile, these second-century problems seem to have been chronologically extended and thus more than just a demographic blip. A register of house occupants from Oxyrhynchus from 235 CE suggests that about fifty per cent of houses were unoccupied.[19] Although dereliction was a normal part of the life-cycle of an urban plot, the scale of abandonment implies a recent fall in population in this area of Oxyrhynchus. The relative absence of mid-third-century documentation from many of the marginal villages of the northern Fayum, Soknopaiou Nesos, Karanis and Philadelphia, means that the transition from the abundantly attested situation of seeming prosperity in the early third-century to one of seeming decline and decay attested in the fourth-century archive of Aurelius Isidorus is little understood.[20] The long and very drawn out deaths of these villages may have been a result of the gradual failure of the water-supply to the villages, possible commencing in the early or mid third century and lasting until the early sixth century.[21] There is, however, no obvious explanation for the collapse of the canal system and there is a possibility that minor geological changes may have encouraged an alteration in the settlement pattern in the Fayum.

There is other evidence for a 'crisis' in the third century. Councils appear to have had some difficulty in recruiting magistrates and magistrates reacted against the burdens imposed on them, though it is not clear to me (rather contrary to the secondary literature) that such reluctance to serve in magisterial positions amounts to a general loss of faith in metropolitan government or a decline in the public spirit of the urban aristocracy.[22] There may have been difficulties

Duncan-Jones (1996), who argues that the plague had a very significant impact on the economy of the empire.

[19] *P.Oslo.* III 111. Only a section of one of the approximately twenty-eight districts of the cities is registered in this survey.

[20] Bagnall (1985).

[21] I argue Alston (1995) 119 that the archaeological reports compress the history of Karanis and that the village remained a centre of population into the fifth century and possible beyond. See also now Pollard (1998).

[22] Skeat and Wegener (1935). *SB* V 7696; *P.Oxy.* XLII 3064; *P.Oxy.* XLIV 3286; *Stud.Pal.Pap.* XX 54; *P.Leit.* 8 = *SB* VIII 10200; *P.Oxy.* XXXI 2569; 2612;XXXVI 2854; X 1252 [*BL* VII, 136]; XII 1414; 1415; Carrié (1976); Millar (1983); Garnsey (1998) 3–27.

with the food supply in the years around 250 CE.[23] The mid-third century also saw administrative reforms such as the abolition of the office of *amphodogrammateus* (urban district scribe) to be replaced by the *phularchos* (*c.* 245) and the introduction office of *dekaprotos* (a rural office held by former senior urban magistrates) and the komarchy (village chiefdom).[24] The 270s also saw a sudden destabilisation of the currency.[25] It seems difficult to deny that this material attests some form of crisis in the mid-third century which may have significant short-term effects.

Nevertheless, one expects ancient societies to undergo occasional temporary crises in which the normal forms of insurance (extensive storage of agricultural produce, for instance) fail to protect the population. The significance of such crises lies in the extent to which they alter the fundamental structures of a society. In many cases, given time, the population simply recovers and is structured in much the same way as before the crisis. The evidence from Egypt appears to suggest that the population recovered in most areas fairly quickly. There does not seem to have been any long-standing fall in the prosperity of urban communities in the late second and third centuries and the centres of many urban centres were adorned with Classical buildings during this period.[26] There is no direct correlation between economic prosperity and a rising population but it would seem unlikely that this pattern of urban expenditure could co-exist with a period of low population.[27] *P.Oxy.* XLIV 3300 of 270 suggests a much higher level of occupancy of houses than a generation earlier at Oxyrhynchus, though it is possible that occupancy levels did not match those of the first century CE.[28] A similar picture can be drawn from early-

[23] *P.Erl.* 18 (*BL* III, 52); *P.Oxy.* XLII 3048; *P.Brooklyn* 18; Rathbone (1996, 1997).

[24] Thomas (1975); Turner (1936); Parsons (1967).

[25] See n. 23.

[26] Alston (1997b); Lukaszewicz (1986); Bowman (1992).

[27] It is argued below that low population often brought economic benefits, but those benefits accrued to the agricultural producers and wage earners since the reduction in competition for work and land and possibly a shift to concentrate on the exploitation of the better quality land placed them in a rather better economic position and conversely squeezed the profits of the elite. Since the elite were responsible for financing major public building projects, one would not expect major public expenditure in the context of a low population.

[28] I argue in Alston (1997a) that there was a large number of persons per house on the evidence of multi-household occupation in first-century texts but *P.Oxy.* XLIV 3300, the only third-century text available, is something of an exception to this

fourth-century registers from Panopolis.[29] These suggest that less than five per cent of all plots were unoccupied or derelict. If there had been a third-century slump, Panopolis appears to have recovered by the early fourth century.

Demographic change from the fourth century: catalogue of sites

There are substantial problems in evaluating the archaeological and papyrological material from sites in late Roman, Byzantine and early Islamic Egypt. Although much of the pottery of the period before the seventh century is distinctive, the sequencing of pottery for the early Islamic period has only recently become clear. The rather peculiar archaeology of Egypt also hampers research since the threat to sites has tended to come from systematic destruction by hunters for *sebakh* and many sites had their uppermost layers removed before systematic archaeological investigation. The period also traverses an important scholarly division in that many of those interested in Roman and late Roman Egypt are not especially informed about the Islamic period. Some of the blanks in the published archaeological record for the transitional period may reflect the relative disinterest or ignorance of the excavators rather than a real absence of material. Similarly, the difficulties of Coptic and Arabic papyrology mean that many are forced to either concentrate on Greek material or to end their periods of study as the Greek material disappears. As a result, making the intuitive leap to treat an absence of (published) evidence as conclusive evidence of absence, especially given the modern preference for 'keyhole archaeology', or to attribute a relative absence of evidence to a change in the density of settlement is an act of historiographical bravery.[30] This probably means that

pattern. I underestimated the fluctuations in average household populations and am now persuaded by medieval evidence, see Herlihy (1973), that numbers of residents per house vary radically according to the demographic trajectory of a community. Evidence from Africa and Syria, the provinces one would expect to be most similar in development to Egypt, suggest that the third century was a period of temporary 'retrenchment'. See Dietz *et al.* (1995); Therbert (1983); Tate (1992) 170–1. Wightman (1985) suggests that there was a more dramatic downturn in the North West.

[29] *P.Berol Bork.*

[30] For the methodological problems in interpreting the evidence from small excavations in relation to broad socio-economic change, see Balzaretti (1991).

only when a site is virtually abandoned will we be able confidently to identify its decline. Such problems need to be considered site by site but a summary of this information is provided in Table 2.

Table 2: Settlement histories from the fourth century

Settlement	Type	History from the 4th to 7th century	Period of decline or abandonment
Alexandria	City	Prosperous until 7th century.	Archaeological evidence of decline in the 8th century
Marea	City	Increased prosperity in the 4th century	Little evidence of settlement from the 7th century
Naukratis	City	Prosperous from the 4th to the 7th century	Little post-7th-century material
Buto region	Villages	Occupation of marginal areas from the third to the 8th or 9th century.	Deserted after 8th or 9th century
Thmouis	City	Reached maximum extent *c.* 450–650	Deserted in the early Arab period.
Athribis	City	Reached maximum extent in the Hellenistic and Roman periods(?)	Prosperous until 5th century and occupied until the 8th century.
Eastern Delta	Villages	Reached maximum density of settlement 300–700	—
Pelusium district	Suburban sites	Occupied from early Roman period to the 6th century	Abandoned in the 6th century
Babylon-Fustat	City	Occupied from the early Roman period.	Rapid growth in the late 7th and subsequent centuries.
Ptolemais Euergetis	City	Prosperous in the 5th–7th centuries	—
Oxyrhynchus	City	Continuous settlement and urban development throughout 4th–6th centuries	—
Oxyrhynchite	Villages	Some changes in settlement patterns in the 4th–6th century.	—
Hermopolis Magna	City	Continuous settlement and urban development throughout 4th–6th centuries	Limited Islamic presence but abandoned by the 11th century.
Antinoopolis	City	Continuous settlement and urban development throughout 4th–6th centuries	—
Thebes	Region	Continuous settlement throughout 4th–8th centuries	Abandonment 810–20

Table 2: (*cont.*)

Settlement	Type	History from the 4th to 7th century	Period of decline or abandonment
Syene-Elephantine	City	Continuous settlement and urban development throughout 4th–7th centuries	Desertion *c.* 750–*c.* 820
Kellis	Village	Occupied in early 4th century	Abandoned in last decade of 4th century or early 5th century.

Alexandria

Alexandria has been continuously occupied from the time of its foundation to the modern day. The Christian literary evidence points to a vibrant and indeed violent religious life in the city in the third to fifth centuries.[31] It does not, however, provide substantial evidence for changes in the total population of the city. Epiphanius suggests that the old palace quarter of the city remained unoccupied after its destruction in the third century, a veritable internal desert, and the building of Christian institutions in urban gardens also suggests that there was not an extraordinarily high pressure on land.[32] Literary depictions of the city stress its grandeur and this *topos* continued into the Arab accounts of the city.[33] The early seventh century saw four sieges of the city, by the Persians, the forces of Phokas, and the Arabs (twice) and Christian attempts to recapture the city may also have caused damage.[34] The *Life of John the Almoner* (see below) suggests a fairly vibrant urban community in the early seventh century in which the Church had considerable wealth, and although poverty was obviously a problem, and this may have been exacerbated by refugees from the Eastern wars flocking to the city, the charitable activities of the bishop do not seem out of keeping with earlier periods, allowing for hagiographic exaggeration.

[31] Haas (1997) provides an account of the religious disturbances in the city.

[32] Epiphanius, *Liber de Mensuris et Ponderibus*, 9 (PG 43, col. 249a–252c); cf Ammianus Marcellinus, 22. 16. 15. Wipszycka (1994). There may have been good literary and religious reasons for Christian authors presenting cities as internal deserts.

[33] Strabo, 17. 1 6–10; Dio Chrysostom, *Oratio XXXII: To the Alexandrians* 41; Aphthonius, *Progymnasmata* (*Rhetores Graeci* ed. L. Spengel, p. 47); Achilles Tatius, 5 1–2; *Expositio Totius Mundi et Gentium* 34–7; Ammianus Marcellinus, 22. 16 7–13; Fraser (1951); Heinen (1991); Eutychius, *Annales* (PG 111 col. 1107).

[34] See Haas (1997) 338–51 on this period and also Borkowski (1981) 14–40; Eutychius, *Annales* (PG 111 col. 1062; 1106–7; 1112; 1149).

The archaeological evidence is based on the exploration of a couple of streets in a central-eastern area of the city.[35] Quite luxurious private houses off these streets were demolished in the third or fourth century to be replaced by a large bath house and a theatre.[36] In the early sixth century, a series of small public buildings of uncertain function (they appear to have been meeting rooms of one kind or another) was constructed and the theatre seems to have been rebuilt. The 'meeting rooms' were probably destroyed in the early seventh century and were replaced in the early eighth century by a large courtyard house with animal troughs and perhaps some small shops.[37] The inscriptions from the theatre suggest that it continued in use in the seventh century and may still have been an important building in the early Arab period. The bath house was repaired in the late sixth or early seventh century but probably went out of use soon after. Its decline predates the Arab conquest of the city. The early-eighth-century pottery is all local ware. The site was reused as an Arabic burial ground in this period and continued to be used as such into the thirteenth century.[38] The location of the burial ground within the confines of the Classical city suggests a considerable retraction of the urban area by the eighth century.[39]

Marea

The partially published excavations at Marea, just to the south of Alexandria, uncovered a fourth-century villa and related urban settlement which served partly as a port on the south side of Lake Mareotis.[40] Rodziewicz suggests that the site may have been redeveloped partly in response to the pilgrim trade heading to the

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[35] On this area see Rodziewicz (1982, 1983, 1984, 1988, 1991).

[36] Kiss (1992c). Kolataj, (1992) 49–51 suggests that the baths were repaired in the early fourth century and probably in the late sixth or early seventh century. The hypocaust system fell into disrepair early in the seventh century and the baths were ruins in the eighth century.

[37] Kiss (1992b).

[38] Rodziewicz (1976) suggests that imported wares were common into the mid seventh century after which Egyptian wares became dominant, a picture confirmed by more recent finds, see Majcherek (1992). On the necropolis see Kiss (1992a).

[39] Haas (1997) 338–51 exploits the Arabic sources to show that Alexandria continued to be an important centre until the eighth or ninth century, though Haas accepts that this continued importance was probably in the context of a population decline. Fraser (1981) suggests a much more rapid seventh-century depopulation.

[40] el-Fakharani (1983); Rodziewicz (1988).

shrine of St Menas, though the evidence for this is slight.[41] The fourth century saw the construction of a harbour, a colonnaded street with shops, and a Church. The absence of Islamic glazed wares on the site suggests that it ceased to be occupied or extensively used in the seventh century, though the pilgrim trade must have continued. The Mareotis region seems to have been economically dependant on nearby Alexandria and a decline in that city would have had major effects in the region. A more specific cause of decline could be found in the seventh-century sieges of the city which seem to have resulted in a closing of the main fresh-water canal. If this was the case, the area, which had been subject to long-term geological changes, may have seen a rapid deterioration in the later seventh century.

Naukratis

Excavations at Naukratis, on the Western flank of the Delta, have tended to concentrate on the earliest levels. As yet, no very clear picture has emerged of the later development of the site. A recent survey discovered a large number of fragments of Hellenistic wares towards the north of the site, but very little of Roman or later date. To the south, most of the sherds were of first to sixth century date and the preponderance of diagnostic fragments date from the late sixth or seventh centuries. Peripheral areas showed a mixed pattern with some areas having their largest deposits of fourth- and fifth-century material and others of material from the sixth and seventh centuries. It would seem that the site was at its greatest extent in the Hellenistic and later Roman periods, though conclusions can only be tentative.[42]

Buto Region

The excavations at Buto, in the north central Delta, are again devoted to understanding the earliest period of occupation at the site but a survey of surrounding koms uncovered no evidence of occupation of these koms before the 'High Empire' and continuous occupation until the eighth or ninth centuries. After this, the sites were deserted.[43]

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[41] Rodziewicz (1983).

[42] Coulson, ed. (1996). See also Coulson (1988), and Coulson and Leonard Jr (1981).

[43] Ballet and van der Way (1993).

Thmouis

Thmouis, in the central Delta, was a Ptolemaic site which formed a second urban centre in the Mendesian nome. Hansen's pottery survey suggests that the site reached its maximum occupation in the period 450–650 but does not allow further precision.[44] The literary material mentioning the site dries up in the early fifth century (after 431) and the city is thought to have been in ruins in the early Arab period.[45] The presence of a bishop of Thmouis 744 (disappeared again by 1086) does not necessarily suggest that the city was a living community in the eighth century.[46]

Athribis

Polish excavations at Athribis have gradually uncovered a Hellenistic and Roman city. The publication of the excavation is piecemeal, though very little material appears to date from the after the fifth century.[47] The numismatic material suggests that the site continued to be occupied into the eighth century.[48]

Eastern Delta

An area on the fringes of the Eastern Delta has been surveyed, though the preliminary published report only categorises the material by dynasty (see Figure 1), and does not allow us to establish trends within periods.[49] The excavators believed that the majority of the Roman and Byzantine period sites dated from after 300 CE. The pattern suggests not only continued occupation of the region in the late Roman period, but a far more intense exploitation of the area.[50]

Pelusium

Much of the excavations at Pelusium, on the extreme eastern fringe of the Delta, has been devoted to the study of the hydrology of the

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[44] Hansen, *et al.* (1967). See also Holz *et al.* (1980); Hansen (1965).

[45] Ammianus Marcellinus, 22.16. 6 numbers Thmouis among the largest cities in Egypt.

[46] de Meulenaere and MacKay (1976).

[47] Mysliwiec (1992); Mysliwiec and Rageb (1992).

[48] Ruszczyc (1992); Kryzanowska (1995).

[49] Based on data in van den Brink (1987).

[50] The broad periods used to classify the various sites present considerable methodological difficulties since a shifting population might occupy several sites only briefly within a particular period.

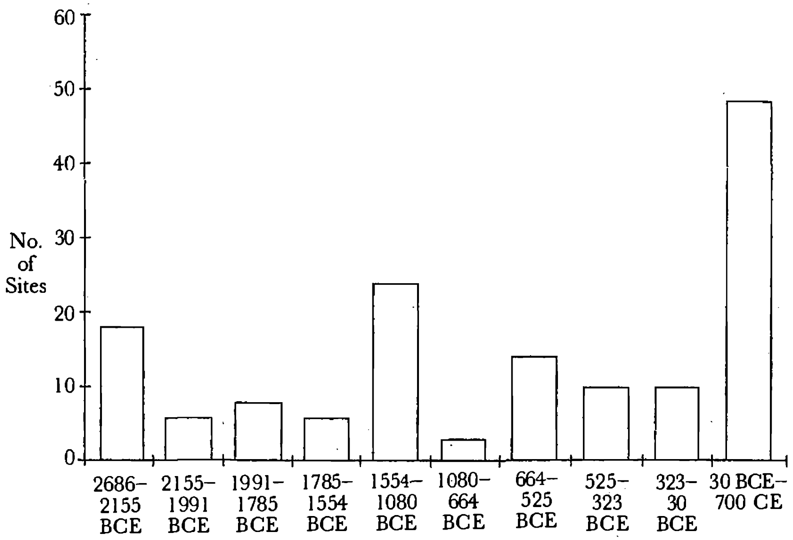

Figure 1: Occupation pattern in the north-eastern Delta.

area. Pelusium was strategically located, the site of at least two forts and was also home to one of the major theologians of late antiquity, Isidore. There is substantial evidence for Arab occupation of the urban site. Thirty-five topographically linked suburban sites produced material from the fourth century BCE to the seventh century CE the majority of which appear to have early Roman origins. There was a fairly small theatre-odeion with a seating capacity of around 2,000, similar in size and style to the theatre at Kom el-Dikka in Alexandria. The theatre-odeion was probably built in the late third century. The excavators would link the construction of the theatre to a major programme of land drainage. In the mid-sixth century, there appears to have been a systematic destruction of the site, possibly part of a salvaging of the remains. Only one of the thirty-five excavated areas contained Islamic material. This desertion is linked to hydrological changes since the success of the aforementioned drainage scheme led to an increase in saline building land but a decrease in agricultural land. The area was completely desiccated in the early sixth century. Purely local factors explain this decline.[51]

[51] Jaritz *et al.* (1996).

Babylon-Fustat

Babylon was developed in the early Roman period and was the location of a major Roman garrison. Most of the archaeological remains of the site are probably either destroyed or are beneath Cairo. The Byzantine camp on the banks of the Nile is one of the few remaining signs of the late Roman city.[52] The site, though, differs from many others in that after the fall of Babylon to the Arabs, Babylon was developed as the centre of Arab military power in the province. The recent excavations at Fustat have shown that the Islamic city was probably a significant centre of population as early as 700. The archaeological evidence suggests that this was new urban growth and not continuity from the old Byzantine city.[53]

Ptolemais Euergetis

Ptolemais Euergetis was extensively remodelled in late antiquity. Twenty-one of the approximately thirty-six districts of the city used in the first four centuries AD disappeared and were replaced by twenty-three new districts.[54] There were at least twelve churches in the city and probably many other Christian institutions.[55] Tax continued to be collected according to urban divisions, suggesting that a fairly sophisticated bureaucracy was maintained.[56] The only possible evidence of decline in the city is the mention of the *epoikiou theatrou* in an urban tax list, suggesting perhaps that the theatre was somewhat separated from the contemporary urban boundaries.[57] All the available evidence points to the prosperity of the site in the late antique period.

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[52] As described by Butler (1978). New excavations are underway at the site, see Sheehan (1996).

[53] Scanlon (1994) suggests that the rapidity of Fustat's development was due to immigration from Alexandria. See also Scanlon (1984).

[54] Daris (1981) and Wessely (1902) IV provide the best published lists and discussion of these districts, though the figures given here are based on an unpublished survey of the *c.* 498 attestations of districts.

[55] *P.Rain.Cent.* 145; *P.Prag.* I 52; 74; 75; 77; *Stud.Pal.Pap.* III 126; 128; 164; 239; 324; VIII 724; 743; 881; X 216; 168; XX 198; 243; *SB* I 4758; 4839; 4891; 4898; 4936; 5128; 5129; 5313; 5134; 5691; XVI 12943; *P.Col.* VIII 244; *P.Lond.* I, pp. 220, 113; *BGU* I 311; *P.Grenf.* I 68; *P.Flor.* III 336.

[56] *SB* I 5127; 5128.

[57] *SB* I 5128

Oxyrhynchus

Oxyrhynchus was Christianised in the fourth and fifth century and markets continued to function in the fifth century but there is little other information.[58] The archaeological record is extremely fragmentary though a very great deal of late Roman/Coptic artistic material was preserved.[59] The Phokas pillar in the centre of the site suggests that it continued to be an important centre into the seventh century and the last Byzantine coins from the site date from the reign of Heraclius.[60]

Oxyrhynchite

Several hundred attestations of villages give something of a data base to assess rural settlement patterns.[61] The material is collated in Figures 2 and 3.

Figure 2 reflects the probable occupation of those villages one might regard as the major rural settlements in the Oxyrhynchite. It is through these villages (grouped into toparchies) that administration and tax collection was organised in the third century. The curve rises to a peak in the third century before a notable fall in the fifth century and later, a fall which appears to have no particular topographical pattern. This curve partially reflects the peculiar chronological distribution of papyri in the Oxyrhynchite. We have very few Ptolemaic papyri but numerous papyri from late first to the fourth century and for the sixth and seventh centuries. The vast majority of these texts were discovered at Oxyrhynchus itself and thus a mention of a village is a measure of the connection between that villages and the city. The later material (fifth century onwards) is dominated by the papers of the Apion family. It may be argued that the fall in the number of villages attested reflects not a demographic shift, but the pattern of investment of the Apions in Oxyrhynchite villages and thus the seeming decline of the established settlement pattern after 400 is illusory. Figure 3, however, makes that case rather less persuasive. All probably occupied villages (including several that were probably rather small) are included in this figure. This

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[58] Rufinus, *Historia Monachum* 5; Modena (1937); *P.Oxy.* VII 1037.

[59] Petrie (1925) 13–17; Grenfell (1896/97); Breccia (1932, 1933).

[60] Bailey (1996), esp. 161–2; Milne (1922).

[61] The smallest attested settlements, which appear to have been little more than farmsteads, have been excluded from the calculations.

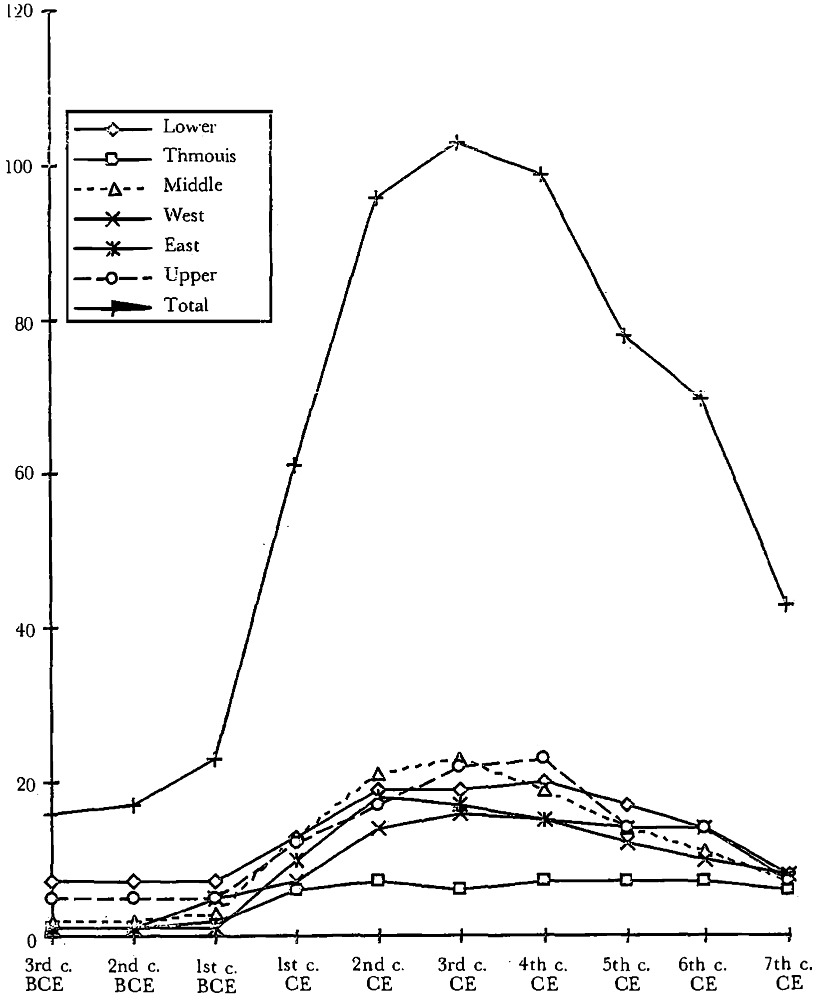

Figure 2: Probably occupied villages of known toparchy by century.[62]

[62] Villages are assumed to have been continuously occupied between first and last attestations.

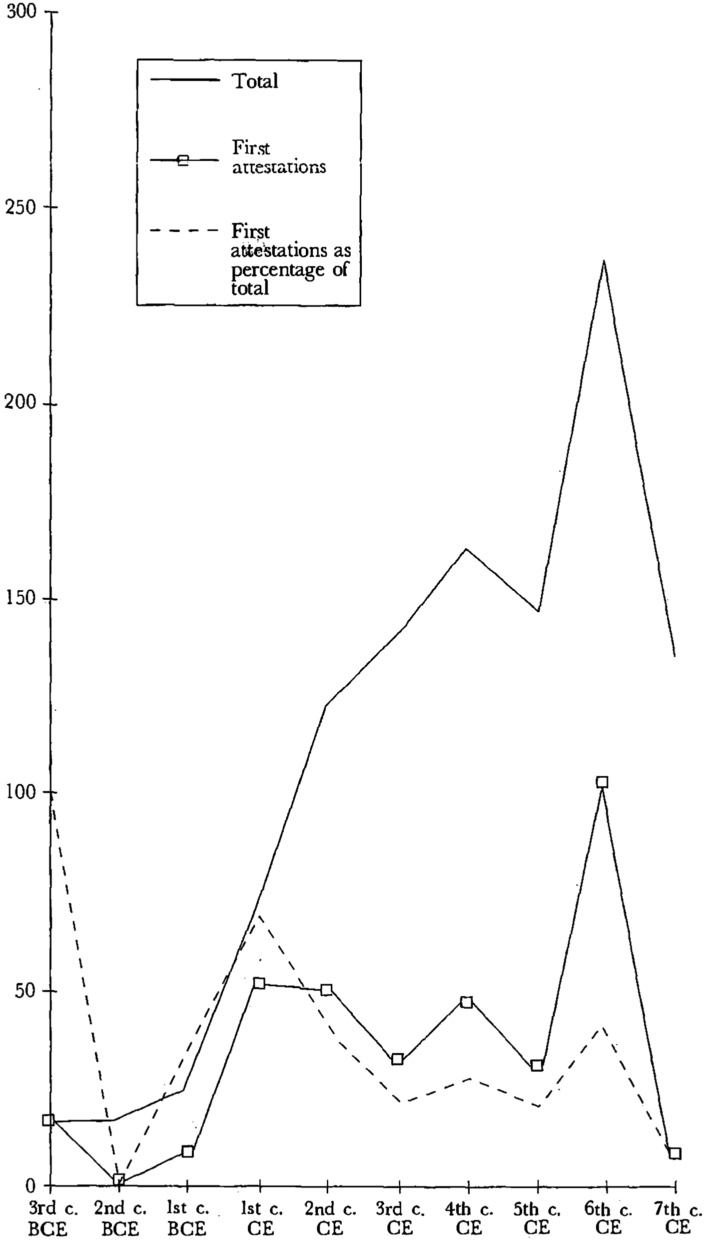

Figure 3: Total of attested villages and first attestations of villages by century.

shows the number of probably occupied villages steadily rising from the first century CE, dropping in the fifth century (which must surely be related to the paucity of papyri from that century), but rising notably in the sixth century. This is a somewhat surprising pattern, contrasting markedly with that of the villages of Figure 2. More telling is the number of new attestations. The proportion of new attestations falls dramatically through the second and third centuries (which one could predict), but although one would expect that trend to continue with new attestations falling to a minimal level in the fourth-century, the number of new attestations rises, both in absolute terms and as a percentage of the total, from the fourth century. Some of the 'new' villages of the fifth and sixth centuries may represent small communities patronised by the Apions or new creations related to the development of the estate. Nevertheless, the patterns of Figure 3 cannot be a result of the Apion estate distorting the statistical base since the rise of the 'new' villages predates the estate's dominance. Figure 3 points to a new pattern of settlement in the Oxyrhynchite taking shape from the fourth century. This shift takes place in the context of the decline of 'established' villages noted in Figure 2. This is a clear example of demographic change though the number of villages attested for the sixth century makes it rather unlikely that this change encompassed an overall demographic decline.

Hermopolis Magna

The British Museum excavations at Hermopolis uncovered a large number of Roman and late Roman structures. Preliminary reports suggest the discovery of a great deal of late Roman pottery, mainly of fourth to fifth century date, though wares of sixth and seventh centuries and some Islamic material have been recovered.[63] Locally produced Hermopolite *amphorae* appear to have circulated until at least the late sixth century and possibly beyond.[64] Earlier excavators found a little Islamic material but the site was probably largely deserted in the early Islamic period.[65]

[63] Spencer and Bailey (1982) 11, for the recovery of Islamic ware from the temple courtyard. Spencer *et al.* (1984) list pottery from the fill on the main road throught the city dating from the third to the sixth or seventh century.

[64] Spencer and Bailey (1982) 16; Spencer *et al.* (1983), Appendix III.

[65] Roeder (1931/32) 122; Bailey (1991) 59.

Antinoopolis

The Antinoopolis excavations in the late 1960s produced pottery dated to the fourth and fifth centuries, though seventh-century and later material was found.[66] The city was still an important administrative centre in the sixth century. I know of no evidence for the history of the site in the Islamic period, though this should not be seen as significant.

Thebes

Wilfong's summary of the material from Western Thebes suggests that there was a very clear terminus for the site of Jeme, reflected in both the archaeology and the extensive textual material, of 810–20. There is no obvious evidence of any prior decline and Wilfong tentatively connects the depopulation of the site with a tax revolt in that decade.[67]

Syene-Elephantine

The papyri and ostraka from Syene-Elephantine show continued urban settlement in the sixth and seventh centuries.[68] Houses were multi-storey and probably densely occupied since the precision of architectural descriptions in house sales and rental agreements suggests that there was a considerable concern over ownership of space and different households could occupy the same house.[69] There was a major Justinianic investment in the city walls of Syene.[70] The excavations in the temple of Chnum on Elephantine have uncovered what was a probably a fort of the fifth century. This fort was converted into a civilian site in the first third of the sixth century. The houses were renovated in the period 575–600 and continued in occupation until at least the last decades of the eighth century and possibly (though there is considerable doubt) into the ninth century.[71]

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[66] Donadoni *et al.* (1974).

[67] Wilfong (1989).

[68] The papyri are in the London and Munich collections. See Farber and Porten (1986) and Clackson (1995) with bibliography there cited and translations in Porten (1996) D20–E3.

[69] *P.Lond.* V 1722; 1724; 1733; *P.Monac.* 8; 9; 11; 12; 13; 14.

[70] Jaritz and Rodziewicz (1994).

[71] Grossmann (1980), with chronology revised by Gempeler (1992).

Kellis

Kellis, a village in the Dakhleh oasis, was probably abandoned in the last decade of the fourth century or the first decades of the fifth. The economy and dynamics of that community, chiefly famous for its diverse and perhaps peculiar religious interests, were distinctly unusual and the economic exploitation of the site was probably marginal.[72] A minor environmental change (movement of a sand dune or the water table) might have had devastating effects.[73]

Conclusions

The nature of the available material almost guarantees that no clear picture of demographic developments will emerge. Yet, there are a number of sites which seem to show increasing prosperity in the fourth and fifth centuries with settlements and probably population reaching levels never before seen. Also we see a dramatic decline by the early eighth century. This would seem to conform to impressionistic views of the archaeology of Egypt. The richness of Egypt as an archaeological zone may reflect changes in settlement pattern intimately related to the hydrological changes that preserved so much material from Roman and Byzantine Egypt. One must, of course, be cautious, since few would argue that the preservation of pharaonic material suggests that Egypt had been in decline since the New Kingdom, yet a comparison of the archaeological record from Hermopolis, which appears to have been deserted, and that from Cairo, which was continually occupied, illustrates the point. This absence of notable medieval or early modern occupation at many important ancient sites suggests fundamental change in the settlement pattern in Egypt by or during the early Islamic period.

Both the date and dynamics of this transition are obscure but it seems unlikely that the transition could have been rapid. The irregularities detected would seem best explained as resulting from a gradual and uneven process. We cannot discount the possibility of this process being underway as early as the very late fifth century but could also opt for a date as late as the Arab invasions in the mid seventh. Although the period abounds in possible causes, economic

[72] On the religious background to the community, see Gardner and Lieu (1996).
[73] Hope *et al.* (1989); Hope (1987); Worp, ed. (1995).

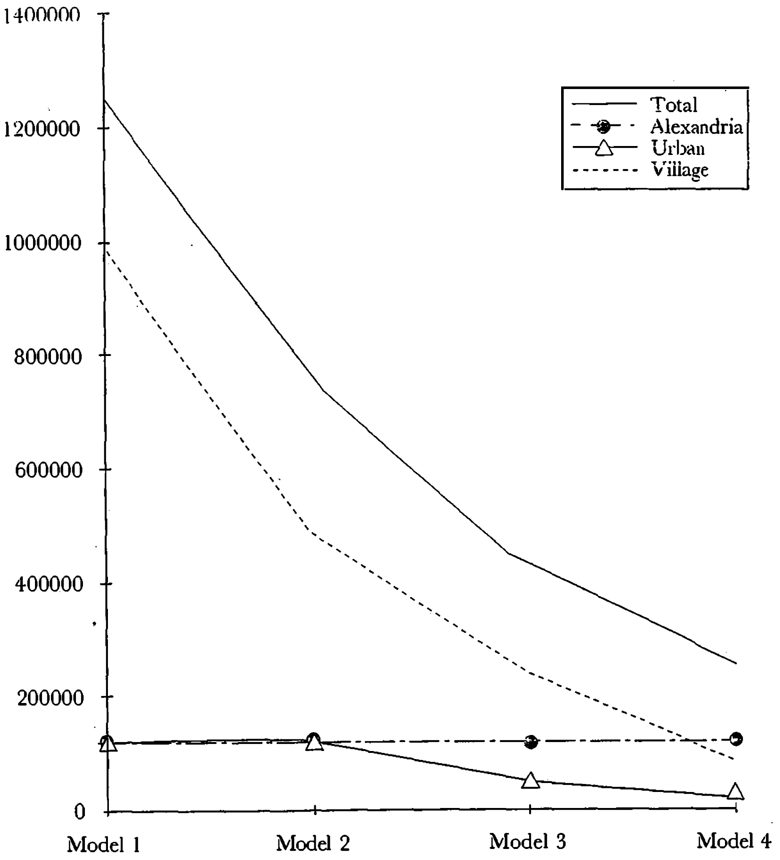
and political division in the Mediterranean, wars, and, most obviously, plague, I suggest that this pattern can only be explained by a reversal of the social and economic conditions that had fuelled growth in the fourth and fifth centuries. Since both establishing changes in economic structure and the causal connection between these and demographic change are problematic, we must first dismiss the other more obvious possibilities before returning to an analysis of socio-economic structures.

Explaining demographic change

Plague

The first known bubonic plague pandemic struck Constantinople in 542.[74] There can be no doubt that the plague was devastating. Our literary sources suggest that mortality was high and that the disease was virulent, appearing probably in bubonic and the yet more serious pneumonic and septicaemic forms.[75] We know of further plague visitations in 558, 573–4, 592, 599, possibly 607, 628, 638–9, 669–70, 673, 686–9, 705–6, 718, 724–5, 729, 733–5, and 745–9 when the plague attacks ended. The historical problem lies in establishing the numbers killed in the outbreaks and the long term demographic impact of the visitations. Conrad calculates on the basis of modern epidemiological models that the plague of 542 may have carried off one-half of the population of Constantinople.[76] Similarly, the plague of 744 at Fustat and Giza is estimated to have killed 48,000, thirty-five per cent of the population. These two outbreaks were particularly violent but Conrad suggests that other attacks may have accounted for twenty-five per cent of various urban populations.[77] However, much of the available material is anecdotal and clearly some of the ancient figures for mortality are merely meant to convey the concept of 'a lot'. Even the cautious approach of Conrad does not persuade: the statistical data is so unsound that the application of

[74] For early accounts of the plague see John of Ephesus, IV 29; Procopius, *Persian Wars*, II 22.
[75] Conrad (1981, 1994).
[76] Conrad (1981) 435.
[77] Conrad (1981) 437–40.



Model Population: 5,000,000 of whom 500,000 are in Alexandria, 500,000 in other urban settlements, 4,000,000 in villages.
All models assume twenty-five per cent mortality in all settlements visited by plague.
Model 1: Plague visits all settlements.
Model 2: Plague visits Alexandria, all urban settlements and fifty per cent of village settlements.
Model 3: Plague visits Alexandria, fifty per cent of all urban settlements and twenty-five of villages.
Model 4: Plague visits Alexandria, twenty-five of all urban settlements and ten per cent of villages.

Figure 4: Mortality of a model Egyptian population in four different hypothetical plague visitations.

epidemiological models is problematic and, more worryingly, recent work on early modern plague outbreaks suggests that 'plague' may often have been a multi-epidemic event since weekly death tolls show patterns that do not fit standard epidemiological models.[78] Conrad's figures, then, should, like those in Table 1, be taken as orders of magnitude with very considerable margins of error. We do not have data on the impact of plague on rural communities in which probably eighty per cent of the population lived.

The problems are increased when assessing plague visitations subsequent to 542 since the geographical spread of epidemics cannot confidently be established. If virulent plague struck every community with frequency and regularity, it is difficult to see how the population could avoid dramatic decline, but if plague struck less frequently in villages than in towns, and there is every reason to believe that this would be the case, then the impact on the total population would be considerably reduced. This can be illustrated by the completely fictional models in Figure 4. These show that changing the assumptions about which communities a plague visitation struck alters total mortality from twenty-five per cent of the population (model 1) to around five per cent (model 4). What is crucial in assessing the impact of plague is not the levels of mortality in a specific settlement, still less the virulence of the disease itself, but the percentage of settlements (especially villages) affected. In a pre-industrial society, one might expect villages to be better insulated from plague than the more cosmopolitan cities.

Plague is primarily a disease of rats and humans. It is spread through personal contact with a victim or by fleas carrying infected blood. Both rat fleas and human fleas appear to be able to carry the virus. Like some other fatal viruses, plague is self-limiting; if it kills all available hosts, then the virus dies.[79] It is important then to assess the 'pool' of available hosts. Transmission will depend on the density of the rat population and its proximity to humans. Human contact with rats is limited by good waste disposal systems and by the use of materials in house construction which offer limited opportunities for rats. Neither were features of ancient communities. But

[78] Champion (1995) 43–4, 64–5, shows that mortality had a curious double peak in various London parishes during the plague of 1665. See also Twigg (1993).

[79] Measles requires a susceptible population of 7,000, suggesting a total community of 300,000–400,000 to become endemic. See McNeill (1977) 60.

transmission of plague must also depend on the level of contact between communities, both of rats and humans. Humans, rats and fleas move along trade lanes and the latter two prefer cargoes of cloth and grain.[80] Plague, therefore, is in some ways a measure of the success of communities in integrating into a larger economic system. If that economic system is destroyed, bringing with it long-term demographic implications, then plague itself should decline. Obviously, the level of integration of the economy and the spread of plague are variables and partial isolation of a community must reduce the chance of plague attack. In such a model, it is theoretically difficult to establish that plague could be responsible for the kind of demographic transition suggested above since if plague did not disrupt the economic system, then there is no reason why the population should not have restored itself, but if it started to have a deleterious effect, the impact of individual outbreaks would be considerably reduced. It is impossible to test such theories on the basis of the ancient evidence, though it has been argued that plague did not cause a substantial demographic change.[81] It is far easier, however, to assess the likely impact of plague against the much better evidence of the demographic results of plague pandemics of the fourteenth and seventeenth centuries.

Medieval and Early Modern Plagues

There has been considerable historical debate concerning the demographic and economic effects of plague visitations. Twigg, at one extreme, has argued from the biological evidence and comparisons with modern plague outbreaks that a disease that killed 0.135% of the population per annum in an outbreak in India in 1896–1917 (where one would have thought environmental factors were particularly conducive to the spread of the disease) could hardly have been a great killer in earlier outbreaks. Twigg argues that the medieval and early modern visitations of plague were 'multi-epidemic events'

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[80] Note that many communities were not affected by the individual plague outbreaks in the early modern era. Indeed, universal plagues seem abnormal. Gottfried (1983) states that fleas can survive without a host for six months to a year if not exposed to low temperatures. Cloth insulates fleas from the cold.

[81] Durliat (1989) suggests that the plague had no serious demographic effects, though the evidence on which the conclusion is based is flimsy. Whitby (1995) argues a similar case based on the ability of the Byzantine to put very large armies in the field in the seventh century.

and thus erodes the belief that the advent of plague is a special biological and historical event.[82] Twigg is undoubtedly correct to point out that the ability of medical practitioners to identify plague was limited and that many deaths might have been falsely attributed to plague. Nevertheless, bubonic plague had an impact on mortality which was of a different order to other epidemic and mortality events in the early modern period. Landers' work on mortality in eighteenth-century London suggests that a cocktail of diseases produced such high mortality that the population could only be maintained through immigration.[83] Yet, although crisis years (years of exceptional mortality) were to be expected, only rarely did non-plague epidemics drive annual mortality to twenty per cent more than 'normal' mortality and never reached thirty per cent more than 'normal' mortality.[84] This contrasts with evidence of mortality in plague years. The outbreak of plague in 1546–7 in certain Devon communities produced mortality rates between three and fifteen times higher than normal. Communities with a small population base are statistically more likely to produce years of exceptional mortality and, in marked contrast to figures derived from the huge population base of London, mortality rates could reach three times the normal even in years when no plague was attested.[85] Plague in Essex seems to have had a more even effect in the late sixteenth and early seventeenth centuries, normally tripling mortality. Yet, fewer than one-fourth of the parishes of the county appear to have been affected.[86] Mortality in urban communities was higher. In Exeter mortality increased in the plague year of 1625 by between 70 and 1160 per cent in the various parishes of the city, though the median increase was about 6.2 times normal mortality. In the 1643 outbreak, the median mortality was 3.3 times more than normal mortality. These

[82] Twigg (1993).

[83] Landers (1993) table 5.4, suggests that life expectancy at birth varied between 17.6 and 28 years from 1730 to 1800. Table 4.10 gives similar figures for the well-attested London Quaker community. Interestingly, this is in line with general estimates for Roman Egypt suggested by Bagnall and Frier (1994) 75–110. Landers 180–3, 174–5 suggests that death rates could have been as much as 0.5 per cent higher than birth rates from the 1730s to the 1770s, though death rates fell dramatically after the 1770s.

[84] Landers (1993), 102–6, 242–300 demonstrates that mortality varied considerably by month.

[85] Slack (1985) 88–90.

[86] Slack (1985) 101–4.

figures represent mortality across all parishes for which information exists.[87] Similar figures for five outbreaks of plague in the years 1565–1645 in Bristol show that median parish mortality rates were between 2.9 and 8.2 times higher than normal. A similar pattern can be seen in Norwich.[88] In London, mortality was also high in the seven outbreaks between 1563 and 1665 with mortality being 2.25 and 7.7 times normal. Plague visitations probably removed around twenty per cent of the population of the city.[89] If we assume an annual death rate for Roman Egypt of 25/1000, a plague visitation might carry off normally about six to twenty per cent (though exceptionally perhaps as much as thirty-seven per cent) of the population of affected communities, provided it followed the sixteenth- or seventeenth-century patterns.[90]

The statistical base for assessing the impact of the fourteenth-century plague is much weaker than that for the early modern period since we have no bills of mortality and few parish registers. Yet, impressionistically, we see a much more significant demographic event. In Perpignan, only forty-five from of 125 notaries and one of nine physicians active before the plague survived, a seventy-six per cent 'disappearance' rate. Higher 'disappearance' rates are recorded in some monastic communities. At Avignon, one graveyard recorded 11,000 burials in a six week period, 140 to 350 times normal mortality. French tax registers suggest 'disappearance' rates of about fifty per cent whereas parish registers in Burgundy suggest mortality twenty times normal levels. Legacies to the Church increased massively in number, though plague may have concentrated minds on the after-life to an unusual extent.[91] In England, there is considerable variation in the figures for mortality, mainly calculated on the basis of manorial rolls and institutional membership (therefore privileging adult males), with figures ranging from nineteen to eighty per cent of the population, though there is something of a consensus in proposing mortality levels of thirty-five to forty-five per cent.[92] These figures are extraordinarily high, far in excess of those proposed for the sixteenth

[87] Slack (1985) 113–9.
[88] Slack (1985) 119–43.
[89] Slack (1985) 144–51.
[90] The death rate is drawn from Bagnall and Frier (1994) 105, using the death rate for those above age 5.
[91] Gottfried (1983) 49–64.
[92] Goldberg (1996); Bolton (1996); Hatcher (1977) 20–25.

and seventeenth century visitations. Since plague deaths were often concentrated in a comparatively brief period within the year, the psychological and cultural impact of the increased mortality must have been emphasized. Such mortality must have convinced that the entire population would be destroyed.

Although once infected, mortality does not seem to have varied significantly, plague manifested itself in different ways and had variable infection rates. The incidence of bubonic (the least serious form), septicaemic and pneumonic plague varied so that although plague is often associated with the warmer months, some outbreaks were maintained, probably through pneumonic transmission, throughout the winter. This variation does not necessarily derive from different biological characteristics. Twigg argues that the plague bacillus has shown no tendency to evolve,[93] though recent reports of the emergence of an antibiotic-resistant strain may cast some doubt on the assertion. Also, although most mammals, other than rats and humans, have some immunity to plague, and this seems to have been the case in the sixteenth century, accounts of the fourteenth-century plague frequently note that other mammals and even birds and fish could be affected,[94] though such cross-species mortality could be explained by hysteria on the part of the witnesses or pollution of water sources, or 'the plague' being a multi-epidemic event. In spite of this anecdotal evidence, environmental factors are a more obvious explanation for variations in death rates than biological changes. Sixteenth- and seventeenth-century mortality varied by district, wealth and gender, though one presumes that the biological characteristics of the disease did not.[95] Environmental improvements in richer areas, not just the tendency of the rich to flee, account for the tendency of the disease to attack poorer parishes in the seventeenth century, though no such pattern can be observed earlier. Nevertheless, the exact combination of biological and environmental factors that might account for the different levels of mortality in the 1347–48 outbreak and those of the later period remains uncertain.

The problem is exacerbated when we consider the social and economic results of plague. Populations tend to recover quickly from cataclysmic events. Marriages are remade and sometimes an easing

[93] Twigg (1993).
[94] Horrox (1994) 7–8; Dols (1977) 156.
[95] Champion (1993, 1995); Slack (1985) 164–9.

of economic conditions (see below) encourages greater fertility. Most authorities argue that for the plague to have a significant demographic effect in the long term, the disease must make frequent and severe visitations, as seems to have happened in the fourteenth, sixteenth and seventeenth centuries. Hatcher suggests that the population of England fell by twenty per cent per generation from 1366 to 1440,[96] though the best evidence for demographic change comes not from England but from the various taxation surveys from Northern Italy. Herlihy shows that the population of rural Pistoia fell from a high in 1244 of *c.*31,220 to a low in 1404 of 8,989. A survey of 1344, three years before the plague reached Italy shows the population at 23,964. Sixty years later, the population had fallen to thirty-eight per cent of its pre-plague level and to twenty-nine per cent of its thirteenth-century high.[97] An extended fall in the population of this scale must have had major social and economic consequences. In Britain, the fourteenth-century plague is related to an economic transformation. The thirteenth century had seen a rise in population. The result was a reduction in the economic power of the peasantry and downward pressure on urban wages. The fourteenth-century population fall led correspondingly to pressure to reduce rents and feudal levies. The increased wealth of the peasantry encouraged more expenditure at markets, urban expansion and economic growth. The initial impact of plague was good for the economy, good for the towns and bad for the landlords. However, the continued reduction of population in subsequent attacks in the fourteenth and fifteenth centuries is said to have caused a contraction in the economy. Lying behind this view is a Malthusian population dynamic whereby England has an ideal level of population which was exceeded in the thirteenth century, reached in the fourteenth century (resulting in economic prosperity), but not achieved in the mid fifteenth century.[98]

The situation in Italy appears to have been a little different. Herlihy argues that population decline was established before the arrival of the plague and attributes the failure of the population to recover after the high mortality of the first outbreak to cultural changes in attitudes to marriage. Although there were some economic benefits in the short-term from plague, it was not until the fifteenth century

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96 Hatcher (1977) 27; cf. Bolton (1996).
97 Herlihy (1965). See also Herlihy (1967).
98 Goldberg (1996); Bolton (1996); Hatcher (1977); Dyer (1991) 14–17.

that social and economic relations stabilised and population levels started to recover.[99]

The demographic developments of the sixteenth and seventeenth century could hardly be more different. Slack guesses that the plague killed 658,000 in England between 1570 and 1670. In spite of this, the population of Exeter rose by perhaps as much as fifty per cent in the seventy years after 1570, the population of Bristol nearly doubled in the 150 years after 1547, and that of Norwich, which had five severe outbreaks of plague, rose from under 17,000 in 1579 to probably over 20,000 in 1640.[100] Most dramatic are the estimates for the increase in the population of London during the plague years. From 85,000 in 1563, the population rose to 459,000 in 1665.[101] The population of the country as a whole probably doubled in this period. This picture of urban growth was not uniform and some centres declined, yet this must be set in the context of increased urban competition and new economic pressures and opportunities to which some towns were unable to respond. England was undergoing the rapid growth in population that came to fuel the industrial revolution and the plague does not seem to have significantly impeded that development.

Conclusions

The fourteenth-century and the sixteenth- and seventeenth-centuries provide two contrasting models for the impact of the bubonic plague. Although it is possible that differences in the biology of the disease account for the different demographic outcomes, it is more likely that these result from social and environmental differences. We are faced with many problems. Standards of hygiene and housing may have been significantly lower in the fourteenth century than the seventeenth, but this is difficult to establish. In Byzantine Egypt, mud brick walls, though better than wood, were probably not an effective barrier against rats and although water was in relatively easy supply and there is plenty of evidence for public baths in urban sites operating until the late sixth century at least, the habit of using abandoned buildings as rubbish dumps probably meant that rat and human interaction was common. Similar problems will have affected

[99] Herlihy (1965). See also Herlihy (1967) 116–151; id. (1970).
[100] Slack (1985) 118–21, 129–32.
[101] Slack (1985) 151. See the broadly similar estimates in Wrigley (1987) 162.

the densely packed rural sites where, although the absolute populations were smaller, population density was not significantly lower.

Plague must have had an effect on the societies it attacked. No population could ignore such severe mortality episodes.[102] Few populations could withstand repeated epidemics taking twenty per cent or more of the population. Yet, both the fourteenth- and sixteenth- and seventeenth-century plagues occurred against a background of socio-economic change. The sixteenth and seventeenth centuries saw the rapid growth of London, with all the concomitant economic changes, and the restructuring of urban economies. This resulted in the decline of some centres and the growth of others. The plague seems to have acted to exacerbate those changes, disturbing urban and (to a lesser extent) rural economies and encouraging population movement. Population growth was undoubtedly slowed in the short term by the plagues, but, in the long term, plague probably speeded up the process of economic and demographic change. A similar case can be made for the fourteenth century. The demographic decline in Pistoia started at least two generations before the advent of the black death. Social discontent and banditry were rife in Pistoia before 1348.[103] There may also have been attempts to limit family size by not marrying or marrying late.[104] The plague made more acute the demographic and social crisis that gripped fourteenth- and fifteenth-century Italy and although contributing to the destabilising of old demographic and economic regimes, does not explain demographic change.

War

The major political events of the seventh century were the wars that were fought largely in the Byzantine empire between the Byzantines and the Persians and the Byzantines and the Arabs. There can be little doubt that these wars ruined some communities and significant weakened others. The destruction of Antioch and the seeming virtual abandonment of many of the cities of Asia Minor after the

[102] The effect of plague on society does not appear as obviously in our admittedly exiguous seventh-century material as it does in fourteenth-century sources. Markus (1997) 4–5 notes the absence of evidence for a renewed spiritualism in reaction to plague.

[103] Herlihy (1967) 209.

[104] Herlihy (1970). See also id. (1973).

Persian wars have encouraged some to see these wars as marking a fundamental break with the late antique settlement pattern.[105] Alexandria probably suffered significantly during the Arab-Byzantine wars of the mid seventh century. Nevertheless, the Persian and Arab wars are not particularly visible in the archaeological record from many sites in Egypt and defeats in the Persian wars did not sufficiently destabilise Byzantine government to prevent it mobilising large armies in the wars against the Arabs.[106]

Egypt was not at the centre of these wars and one might expect that any wholesale destruction resulting from them would be more easily detected in the much fuller archaeological record from Syria and Jordan. There are problems in evaluating this material, but the number of excavations and surveys of Syria and Byzantine Arabia that point to similar developments is reassuring. Surveys in the Jordan valley suggest that the early Roman period saw a growth in the numbered of occupied sites and this may have continued into the post-Justinianic period (though there is some evidence for a shift in the pattern of settlement between the early and later Byzantine periods).[107] There appears to have been slow decline through the Umayyad period but no noticeable or dramatic break. Excavations at Pella have uncovered evidence of decay by the eighth century but the city appears to have been occupied after 747.[108] Excavations at Apamea suggest urban change in the sixth and seventh centuries while those at Bet Shean (Scythopolis) show continuity of settlement into the eighth century but considerable evidence of impoverishment.[109] Similar continuity is argued for Kastron Méfaa and Piccirillo suggests that the city continued to flourish into the late eighth century.[110] King

[105] See n. 2 and the summary of views in Cameron (1993) 152–200.

[106] The evidence of damage in the Persian wars is collected by Altheim-Stiehl (1991, 1992). For the Byzantine resistance to the Arabs see Whitby (1995), and Kaegi (1992). Cameron (1993) 176–96 has a more pessimistic view of Byzantine capabilities arguing that a combination of factors explain the military problems in the seventh century, but that the Persian wars so fundamentally weakened the Byzantine empire that the Arabs were able to secure significant successes.

[107] Yassine, Ibrahim and Sauer (1988a, 1988b) identify sites in the following periods: 25 hellenistic, 55 early Roman, 22 late Roman, 40 early Byzantine, 36 late Byzantine, 36 Umayyad sites. The archaeologists had some difficulty differentiating early and late Byzantine sites 86 sites which showed evidence of Byzantine occupation. Of the 46 Byzantine sites identified in the southern sector of the survey, only 3 showed evidence of both early and late Byzantine occupation, suggesting either an astonishingly mobile population or problems in the archaeological methodology.

[108] Smith and Day (1989) 8–10; Walmsley (1992) 377.

[109] Balty (1989); Tsafrir and Foerster (1994).

[110] Piccirillo (1992).

argues that there was no notable break between the Byzantine and Umayyad period in Jordan as a whole while Johns' reports a notable decline in the number of identified sites in the Umayyad and Abbasid periods in the Ard al-Karak but blames this on the difficulties in sequencing the pottery.[111] A similar picture emerges for the region of modern Syria. Tate suggests that there was a period of expansion in rural settlement from 270–450 which was increasingly vigorous after 330. There was a period of stagnation from the sixth century and then a steady decline but occupation continued into the early eighth century. Decline started before the disasters of the seventh century and was maintained after them.[112] This ragged pattern of continuity or gradual decline through the seventh and eighth centuries is supported by Foss' review of rural settlement in the Byzantine and Arab East.[113] There is, therefore, no obvious break in the archaeological record in the seventh century that might relate to damage caused by the wars of the period.

The pattern of settlement change in Egypt (and, for that matter, Syria) does not reflect a single cataclysmic event, but a process. There is no obvious reason why those Egyptian communities affected by the wars should not have gradually recovered. Wars do not seem a likely cause of the demographic change, though, as with the plague, they probably contributed to the destabilising of the old economic and demographic system. The political result of the wars probably also had little effect on settlement patterns. The new Arab rulers retained many Byzantine administrative structures. Arab policy encouraged urbanism, though the urbanism of the Arab period seems somewhat different from that of the early Roman empire.[114]

Social and Economic Transformations

The papyrological evidence from Late Byzantine and Early Islamic Egypt gives the impression of a society functioning very much as it had done in previous centuries. There were, however, many developments, one of which was the increasing power of the Church. Already in the late fourth and early fifth centuries when the legend

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[111] King (1992), 369–75; Johns (1992).

[112] Tate (1992) 170–85, 303–42.

[113] Foss (1995), who suggests that in the Negev, most sites continued to be occupied through the seventh century but were in notable decline by *c.* 700.

[114] Kennedy (1985).

of Shenoute of Atripe was being formed, his monastery was portrayed as an economically powerful organisation able to (miraculously) support the local population during a famine and engage in other charitable works.[115] The archives from western Thebes suggest that the town of Jeme farmed the lands owned by the local monastic institutions that dotted the hills of the area.[116] The hagiographic literature and indeed the Church histories and the documentary material both Greek and Coptic, are filled with references to Christian building and charitable activity, especially in support of the sick, female or aged. We have anecdotal evidence of this from Leontius' *Life of John the Almsgiver,* in which the seventh-century bishop of Alexandria is said to have expended vast sums on supporting various good causes.[117] John Moschus' *Pratum Spirituale* is a treasure-trove of moral tales often involving episcopal charity and charity is also emphasised in the probably largely genuine *Canons of Athanasius.* The building of cathedrals, such as the large church in Hermopolis, was a measure of the investment of the bishops in the urban infrastructure and there were large numbers of Christian institutions which were partially or wholly funded from the revenues of the Church.[118] There may have been more than forty churches in Oxyrhynchus in the fifth and sixth centuries and probably at least eighteen in Ptolemais Euergetis.

The patterns of urban euergetism may be described as being broadly similar to earlier practice in that the Church inherited the role of urban patron from the town councillors. Nevertheless, one could argue for a shift of focus from a culture centred on the city, as had arguably been the case from the Pharaonic period, to one in which the city was, if not exactly marginal, not at the centre of most religious and cultural activity. Although village temples had been important and powerful centres and the cultural importance of

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[115] Besa, *The Life of Shenoute* 20, 27–30, 32, 90, 139–45.

[116] Winlock and Crum (1926); Crum and Evelyn-White (1926); Godlewski (1986); Khater and Khs-Burmester (1981); Bachatly (1950).

[117] Leontius, *Life of St John the Almsgiver,* 2 (feeding the poor); 7 (aiding refugees from the Persian war); 8 (general charity); 8 (mention of the Church's fleet of merchant ships); 11 (Church receives large donation); 17 (Church owns shops); 20 (Church sends aid to the bishop of Jerusalem).

[118] Wipszycka (1972) details the range of economic activities which involved the Church but very few documents allow any comparison of the wealth of the Church with that of other institutions. On the role of the bishop see also Martin (1998) and Wipszycka (1998).

villages was carried into the Roman and Late Roman periods and bishops wielded considerable power, the number and prominence of monastic establishments in the sixth century argues for a ruralisation of religious attention on a scale never before seen. I have been able to find thirty-three monasteries associated with Aphrodito, twenty-nine monasteries near Hermopolis (compared with thirty-one probably urban churches), and twenty-three monasteries in the Oxyrhynchite.[119]

The enumeration of institutions is not necessarily a good guide to their power or influence. A cadastral document from the village of Aphrodito allows us a more nuanced picture of the power of the Church.[120] The text is unfortunately incomplete and although we have the summary account, giving total land holdings in the village territory, we only have detailed records relating to about twenty-five per cent of that land. The cadastre registers 1070 *arourai* of grain land of which forty-seven per cent was owned by the Church, and 81.5 *arourai* of other types of land, of which about fifty-eight per cent was Church land. Once this is further broken down, about forty per cent of all grain land (eighty-five per cent of Church holdings) and fifty-seven per cent of other land (ninety-six per cent of Church holdings) was owned by monasteries.[121] There is, of course, no guarantee that the remaining land in the village was apportioned in the same fashion as in this cadastre. A small proportion of this land was owned by ecclesiastical institutions based outside Aphrodito, mostly in the nome capital of Antaiopolis, but the majority of the ecclesiastical landholdings appears to have been owned by local monasteries. The surplus of the village was used to support a large number of Christians and Christian institutions in the environs of the village.

The cities seem to have remained centres of exchange, though the evidence from Aphrodito (which might not have been a typical village in that it had aspirations for urban status), suggests a much

[119] The figures here are drawn from a much fuller study of the Christian city to be published as part of a monograph on the city in Roman and Byzantine Egypt. The argument parallels the discussion of the so-called flight of the curiales from city councils which is similarly thought by some to have refocused wealth in non-urban contexts and is blamed for the decline of urbanism in the West. See Liebeschuetz (1992, 1996); Carrié (1976); Millar (1983); Garnsey (1998) 3–27; Whittow (1990).

[120] Gascou and MacCoull (1987).

[121] If 'Apa Dios' is taken as a monastic foundation, then the figures for monastic land are forty-two per cent of all grain land (eighty-nine per cent of Church holdings) and fifty-nine per cent of other land (ninety-eight per cent of Church holdings).

greater involvement in trade and manufacture than is shown by the villages of the Roman period. An undated list of contributors lists 156 males of whom thirty-seven per cent had trade designations.[122] Another partially preserved tax list names eleven guilds active in the village.[123] A further five guilds are attested a sixth- or seventh-century tax list.[124] The numbers of traders active in the various guilds cannot be assessed but it does seem that trade and manufacture played a rather more important part in the economy of Aphrodito than it had in the economy of Karanis four or five centuries earlier.

Another area of possible change is the role of the great estates of late Byzantine Egypt, the most famous being that of the Apions.[125] In spite of the mass of material, both published and unpublished, relating to the workings of the Apion estate in the Oxyrhynchite, there are still many fundamental problems. The Apions spent money on institutions, including their suburban villa, the circus factions, and the baths, and were probably responsible for employing large numbers of people, though much of their euergetism was directed through the Church. One presumes that their example was followed by the other great houses of the city. It is not clear that their behaviour differs notably from euergetistic activities in earlier centuries, though it is probable their expenditure was spread more widely, financing ecclesiastical institutions on their estates and also, presumably, supporting their political activities and households in Constantinople. The Apion estate operated in ways very different from those of great landowners of earlier centuries. The rent roll had replaced the mass of rental agreements with all that implies for the flexibility of the management of the estate and it looks as though the estate management was not as closely involved in the day-to-day farming of its land.[126] The Aphrodito archives suggest that landowners were generally remote from the village and both ecclesiastical and secular landowners conducted their business mainly through agents and subtenants who formed the powerful in the village.[127] If the Aphrodito

[122] *P.Cair.Masp.* III 67288.

[123] *P.Cair.Masp.* III 67147.

[124] *P.Hamb.* I 56.

[125] See on this estate Hardy (1931) and Gascou (1985).

[126] See Rowlandson (1996) 283–4, on tenancy. For estate management see Rathbone (1991).

[127] Keenan (1980, 1984, 1985). For management of ecclesiastical land at Aphrodito see what appears to be a rather careless contract of sale in *P.Michael* 41 and also *PSI* VIII 936; 937.

archives are seen as typical, they represent a devolution of power from the city to the village. Similarly, the concentration of property in the hands of a relatively small number of families, as attested in the Apion archives, also represents a shift in economic power away from the *curiales*.

The village elite and the Church may not have been the only beneficiaries of changes in political and economic structures in the Byzantine period. The state may also have grown in authority. Gascou has argued that the traditional interpretation of the Apion archive as evidence of a feudalisation of agricultural and political relations is misleading. We should not see the Apion estate as a commercial enterprise but rather as an arm of the state.[128] As late as the seventh century, according to Leontius, imperial officials were sufficiently powerful that they believed they could raid the coffers of the Church.[129] The state continued to have a role in the enforcement of law. Although other forms of dispute resolution became popular from the fifth century, the courts still existed and Byzantine law could make itself felt in the villages and cities of Egypt.[130] Justinian's pronouncements on taxation cannot be dismissed as rhetoric and Procopius claims that his rulings were influential (if detrimental to the Byzantine empire) but his ability and that of his successors to finance and support armies in the field is a concrete manifestation of state power.[131] It was the late seventh century that saw the collapse and reformation of the Byzantine state in the face of catastrophic military defeats against the Arabs, not the sixth century.[132]

In the sixth century we see a powerful state, comparatively powerful villagers, a powerful church, and possibly powerful landowners. Much of this power seems to have been taken from the city and from the old bouleutic elite. Although no-one would suggest that the old elite simply disappeared from the urban scene in the East, the curial elite is less prominent in the sixth-century documentary material

[128] Gascou (1985) *contra* Hardy (1931).

[129] Leontius, *Life of John the Almsgiver*, 12.

[130] See, for example, the case in Gagos and van Minnen (1994), though *contra* the editors' interpretation. See my review in Alston (1996).

[131] Justinian, *Edict* 13; Procopius, *Anecdota*, 21–3, discussed some of the more extraordinary finance-raising measures alleged to have been adopted by the emperor. Whitby (1995) discusses the ability of the Byzantine state to raise troops.

[132] It is, of course, fruitless to wonder whether the state was more or less powerful in the late sixth century than it was in the third. There is no reliable measure for state power.

than in that of the fourth century and it is very tempting to see this virtual disappearance as a reflection of the elite's increasingly marginal political and financial role. This period saw the power to collect taxation finally removed from town councils.[133] The development of new and powerful extra-conciliar offices, such as those of the pagarch, the *pater poleos*, and even the bishop granted administrative authority to what the legal texts appear to call the *possessores* (the major landowners) rather than to senior members of the curial group.[134] Such individuals were no longer necessarily identified with the city and perhaps operated from the large suburban villas, such as that of the Apions.

We see in the fifth and sixth centuries a gradual erosion of the political and cultural centrality of urban centres. Clearly, this did not necessarily mean the end of the ancient city: many cities retained trading or administrative functions, probably continued as centres of elite and ecclesiastical consumption, and remained centres of population. Nevertheless, I suspect that these continuities were not sufficient to offset changes in the cultural and political institutions of the Byzantine period and these gradually undermined the viability of cities. We thus have an important structural change in fifth- and sixth-century Egypt that undermined the centrality of the cities in the ancient settlement pattern long before the shocks of plague and war accelerated processes of urban change. Since the cities formed part of an integrated settlement system, which generated a certain prosperity in the Roman and early Byzantine periods, the decline of the hubs of those networks eventually threatened the entire of the settlement system. Some centres were better placed to cope with these threats and some, notably Fustat, may have prospered in the new conditions of the seventh century, being able to establish themselves as central places of some importance. Many others declined within the settlement system so that cities became villages, or simply faded away. The erosion of the network of local *poleis* as economic, religious and political central places was, I believe, the crucial causal element in the demographic developments of the late Byzantine and Early Islamic period.

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[133] Delmaire (1996); Lepelley (1996).

[134] Holum (1996); Liebeschuetz (1973, 1974) Feissel (1987); Roueché (1979); Sijpesteijn (1987); Rouillard (1928). These offices seem rather different from the fourth-century 'state appointees', the *logistes* (*curator civitatis*) and *ekdikos* (*defensor civitatis*) who appear to have been drawn from the bouleutic group. Kramer (1990); Lallemand (1964); Rees (1952, 1953/54).

Conclusions and other provinces

The events of the fifth to the seventh century, a period which has been seen as marking the end of antiquity, have attracted several bold analyses. Since Pirenne made the issue the focus of debate for both historians of late antiquity and those interested in the new urban centres of the early medieval period, historians and archaeologists have become aware of the divergent histories of various cities in the late antique and early medieval periods and the archaeology of the late antique city has become available. The period from the fifth to the seventh century is now so much more complex and multifaceted that the simplicity and clarity of Pirenne's thesis (at least in outline) must elude us. The inability of historians and archaeologists to agree about when and if the ancient city disappeared encourages scepticism as to whether there should be a general theory of change. Some recent commentators have chosen to emphasise continuities,[135] perhaps reacting against those historians who have tended to operate with a very static model of ancient urbanism and, especially when writing about late antiquity, have equated all change with decline. Yet, even given the 'ragged' chronology, the clustering of changes in what had been a comparatively stable and long-established settlement pattern in most areas of the Eastern Mediterranean in the period from the sixth to the ninth century (perhaps predated in the West by one or two centuries) seems to demand a general explanation. The chronological differences in the patterns of decline across the empire seem to exclude cataclysmic events as primary causes for the change and this would immediately lead to a consideration of long-term socio-economic factors. Any such explanation must focus attention on the late antique city. It is a reasonable assumption that the factors that explain the prosperity of Roman and late antique settlements might be related to the cause of any decline. However, the nature of the ancient city remains an issue of fierce historiographical debate. Without an agreed theory to explain how the ancient city worked, explaining how it stopped working becomes much more difficult.[136]

This theoretical difficulty helps account for the different theories of long-term socio-economic change offered in explanation for the

[135] Barnish (1989).
[136] See the similar point raised by Ward-Perkins (1996).

transition. Pirenne, for instance, believed that the heart of the ancient city lay in trade, especially long-distance trade.[137] In this, he was in keeping with the dominant contemporary historical perceptions of ancient and early modern urbanism. Those who still value the Pirenne theory tend to be interested in the re-emergence of urbanism, especially from a North-European perspective,[138] though other models of early medieval urbanism have been proposed and it seems possible that a diversity of urban types may have emerged in the West (and no doubt the East also) in the early medieval period.[139] For ancient historians writing after Finley, Pirenne's characterisation of the ancient city as being dependent on long-distance trade seems almost quaint.[140] Yet, although I think few would now take seriously such a model for most ancient urbanism,[141] Pirenne's thesis shows the interdependence of our explanations for change and our views of antique socio-economic structures.

Wickham argues that late antiquity saw a gradual shift in socio-economic structures from the 'dominance' of an ancient mode (in which the state extracted considerable proportion of surplus production) to the 'dominance' of a feudal mode (in which landlords concentrated the surplus in their hands and the claims of the state were considerably reduced). The erosion of the state by the elite, who used their powers of patronage to offer protection from state imposts to villagers, fundamentally changed social and political structures.[142] Further, this erosion of the state is linked to the perceptible decline in long-distance trade in the sixth century since, Wickham argues, the development of long-distance trade was a peculiar phenomenon intimately related to the development and extension of Roman state power, especially through taxation.[143] Incidentally, the Roman state's demands for supplies for urban populations and for the armies sponsored the development of a trading infrastructure. The collapse of Roman power in the West led to that collapse of

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137 Pirenne 1925, 1939).

138 Hodges (1982) gives a far more sophisticated and developed version of the Pirenne thesis. See also Hodges (1996); Hodges and Whitehouse (1983).

139 See most recently Halsall (1996); Balzaretti (1996).

140 See the witty and very brief demolition of Pirenne (and French and German nationalistic sholarship on the *Volkwanderung*) by Wickham (1989), and, more seriously, Barnish (1989).

141 Though see Engels (1990).

142 Wickham (1984).

143 Wickham (1988, 1989).

the infrastructure and of taxation and (limited) economic decline followed. This is a familiar model: a reversal of Hopkins' explanation for the growth of trade and the economy in the first centuries CE.[144] It is also a mirror image of the explanation for the transition I offer above. Where Wickham differs from Hopkins is in his assessment of the economic importance of long-distance trade. For Wickham, this is a marginal factor in the ancient economy since ancient cities depended on their ability to exploit their territories and this was unaffected by the collapse of the Roman state and the subsequent political and military changes. Such 'primitivist' models of the ancient economy emphasise continuity and see urban growth or decline as problematic. Yet, the discontinuities in the evidence at ancient sites would seem to pose problems. Wickham argues that these discontinuities are features of the evidence and not of the urban centre: an absence of trade goods after the collapse of the Roman trading system makes fifth-century sites less easy to identify and political uncertainty prevented aristocratic investment in urban sites, again leading to their invisibility. The cities remained the same, they just looked different. Since the absence of elite spending in cities and the breakdown of long-distance trade networks become economically irrelevant, much of the physical evidence for the community is effectively abstracted from its role in the local economy. This has its attractions in that historians have discussed the abundant evidence for changes in urban morphology in late antiquity but have generally concluded that this had comparatively little effect on the status of communities as urban centres.[145] The basic economic functions of the settlements remain unchanged. Similarly, the impoverishment of urban centres in the late Byzantine period or the early Islamic period could be seen as being of no general economic significance since it simply means that the surpluses generated in the countryside were going elsewhere.

Nevertheless, the evidence from Egypt and the East suggests that the problems of the period are not just related to the disappearance of evidence (though this may contribute significantly to our pessimistic picture of early Arab-period settlement), but are caused by a fundamental re-orientation of both urban and rural settlement patterns.

[144] Hopkins (1980b, 1995/96). See also a similar model proposed by Rozman (1976) 72–3.
[145] Cormack (1990); Alston (1997).

The evidence of an increased density of settlement in rural areas in the Roman and Byzantine period is crucial since it suggests that the settlement pattern and economy of these areas was not static. In the first section of this paper I outlined a model of urban-rural interaction in Egypt which generated economic specialisation and growth. My explanation for the demographic transition at the end of antiquity depends on the prior acceptance of that model as Pirenne and Wickham rely on their models.[146] It is not the case that the various alternative models should necessarily be opposed: I argue elsewhere that the manifest differences in cultures, histories, urban-rural interaction and location within trade networks mean that a unitary conception of 'the ancient city' is overly crude and although the cities of the late Roman and Byzantine empire did tend to have many similarities in cultural and social structures and, of course, responded to similar economic and political pressures, the differences in the histories of various urban communities across the empire suggest that the ancient city or even Roman and Byzantine cities were not the same wherever the phenomena appeared.[147] This diversity explains why different settlements and settlements systems responded differently to the cultural and political changes in late antiquity. Nevertheless, we have seen that the patterns of change in Egypt and Syria appear to be broadly similar and this pattern appears to emerge from the archaeology of other provinces, especially Africa.[148] We thus, I think, need an explanation for which relies on widespread social and cultural change, but can account for a diversity of results in settlement systems. Unlike Wickham's picture, where the decline of the state undermined the late antique trading system, I argue that it was the triumph of the state and Church over the local aristocracies that led to the gradual decline in the cultural, political and economic cen-

[146] Kennedy (1985b) suggests a rather similar explanation for the demographic changes in Byzantine and Arab Syria.

[147] See n. 3.

[148] See the dramatic changes in settlement pattern uncovered in Dietz *et al.* (1995) 773–86, where the number of occupied sites rose from 7 in the period from AD 1–50 to 26 (250–300) to 59 (400–450) to 83 (500–550) before declining to 12 (600–650) and 3 (650–700). Barker and Mattingly (1996) produce a rather more complicated picture of settlement pattern change at the end of the Roman period and the beginning of the Islamic period, though there does appear to be a very notable decline in settlement density in that period. There is evidence for changes in Italian settlement patterns in the sixth century which one might wish to connect to these Eastern changes. See Hodges (1993–95, 1997); Hodges and Bowden, eds. (1998) with references to other literature.

trality of the cities. Since, in my model, the growth of Classical urban systems integrated the economies of various cities and villages and was crucial to economic development, the decline of parts of that system (the cities) lead to the decline of the whole settlement system.

Egypt shows little evidence of this transition before the seventh century. Some continuity of urban settlement into the seventh and eighth centuries is also to be expected and is confirmed by the evidence from Egypt and the rest of the East. This archaeological material does, however, suggest changes in the nature of urban centres: an 'Islamicisation' of the Classical city before the arrival of the Arab invaders.[149] Historians have seen this as evidence of change not decline, in the same way as the Christianisation of cities a century or so earlier clearly did not mark the end of ancient urbanism.[150] Some cities in Syria continued to maintain quite elaborate urban facilities. If Leontius' *Life of Symeon the Fool* is to be taken seriously as a reflection of sixth- or seventh-century urban life, there was not only a diversity of entertainments on offer, but also a range of commercial enterprises and at least two public baths. Nevertheless, the archaeological evidence for the impoverishment of the public cultures of Bet Shean, Apamea and Antioch must be interpreted within the changing economic system.[151] These centres remained urban, but they were losing that economic power and centrality that had driven and supported the late antique economy. This steady erosion of the settlement pattern was undoubtedly speeded up by the traumas of the sixth and seventh centuries.

New structures developed in place of the old settlement systems. The early Islamic period saw the growth of new urban communities, especially Baghdad, Basra, Medina, and Fustat. These were new political and economic foci, centres which established trade and other networks that allowed them to survive and grow in spite of the undoubted demographic shocks of plague. The success of these centres

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149 Kennedy (1985a). Claude (1969) and Bouras (1981) note the erosion of clear urban plans in the sixth century.

150 Cormack (1990): See, however, the interesting argument of Saradi (1998), who suggests that the evidence of the division of large urban properties into smaller units implies a decline in the urban aristocracy who were no longer able to maintain grand residences. Although this would tend to support the thesis outlined here, I am not convinced that the abundant evidence of the rental of rooms in urban houses suggests much more than urban residents exploiting their assets.

151 Balty (1989, suggests that only in the early seventh century was there sign of economic change. Tsafrir and Foerster (1994). Kennedy (1985a,b).

argues against theories of cataclysmic change, yet their very emergence confirms that this was a new world. The settlement pattern and economic system of the previous centuries had been replaced by new centres of urbanism. No such transformation can be without continuities and some centres were better placed to adapt to the new patterns than others. Nevertheless, the changes in the settlement pattern were sufficiently radical (if gradual) that we are justified in seeing in this transition the end of the ancient world.

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NOTES ON CONTRIBUTORS

Richard Alston is a Lecturer in the Department of Classics, Royal Holloway College, University of London. His previous publications include *Soldier and society in Roman Egypt* (1995), *Aspects of Roman history AD 14–117* (1998), and a series of articles on urbanism in Roman Egypt and military life. He is working on a volume on urbanism in Roman and Byzantine Egypt for publication in 2001.

Bruce W. Frier is Professor of Classics and Henry King Ransom Professor of Law at the University of Michigan in Ann Arbor. His principal areas of specialisation are Roman law and Roman social and economic history. He is the author of *Libri annales pontificum maximorum: the origins of the annalistic tradition* (1979), *Landlords and tenants in imperial Rome* (1980), *The rise of the Roman jurists: studies in Cicero's* Pro Caecina (1985), *A casebook on the Roman law of delict* (1989), and the co-author of *The demography of Roman Egypt* (1994, with R. S. Bagnall) and *The census register P.Oxy 984: the reverse of Pindar's Paeans* (1997, with R. S. Bagnall and I. C. Rutherford). He has also published numerous articles on Roman law, history, historiography, numismatics and archaeology.

Elio Lo Cascio is Professor of Roman History at the University 'Federico II' in Naples. A former student of Santo Mazzarino, he graduated from the University of Rome, subsequently taught at the Universities of Lecce and L'Aquila, and spent several terms doing research at the University of Cambridge and the Institute for Advanced Study in Princeton, where he was a member of the School of Historical Studies. His published work primarily focuses on three topics: the administrative history of the Principate and the Late Empire; the economic and social history of Rome, with particular emphasis on monetary history from the Late Republic to the Late Empire; and Roman population history and the impact of demographic change on the economic and social history of Rome. His most recent publications include *Il* princeps *e il suo impero: studi di storia amministrativa e finanziaria romana* (2000) and the edited volumes *Roma metropoli preindustriale* (2000) and (with D. Rathbone) *Production and public powers*

in antiquity (2000). He is the editor of the series *Pragmateiai: texts and studies in the economic, social and administrative history of the ancient world.*

Walter Scheidel teaches ancient history at the University of Chicago. Formerly Moses and Mary Finley Research Fellow of Darwin College, Cambridge, he has also taught at Stanford University, the Ecole des Hautes Etudes en Sciences Sociales in Paris, and the Universities of Vienna, Graz and Innsbruck. He is the author of *Grundpacht und Lohnarbeit in der Landwirtschaft des römischen Italien* (1994), *Measuring sex, age and death in the Roman empire: explorations in ancient demography* (1996), and *Death on the Nile: disease and the demography of Roman Egypt* (forthcoming), and has published articles on a variety of subjects, especially ancient social and economic history. The editor of the collected papers of Peter Garnsey (1998), he is co-editor of the forthcoming *Cambridge economic history of the Greco-Roman world* and of forthcoming collections of essays on the ancient economy, ancient empires, and pre-modern demography.

Brent D. Shaw received his PhD from Cambridge and is at present Professor of Classics and Chair of the Graduate Group in Ancient History at the University of Pennsylvania. His research has diverged into three rather separate areas of Romans social history: the regional history of the north African provinces as part of the Roman imperial system, the problem of civil and military violence in the empire, and the history of the Roman family, especially its demographic aspects. He has published in all areas in monographs and leading scholarly journals. A number of his papers have been collected in *Environment and society in Roman North Africa* (1995) and *Rulers, nomads and Christians in Roman North Africa* (1995). He is also known as the editor of M. I. Finley, *Economy and society in ancient Greece* (1981, with R. P. Saller), and of an expanded edition of Finley's *Ancient slavery and modern ideology* (1998). He is currently working on a sourcebook on the Spartacus slave war and a monograph on the violent gangs in north Africa of the later empire who were known as *circumcelliones.*

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